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WISDOM AND KNOWLEDGE:

AN ADDRESS

DELIVERED AT THE

Stoke Newington Mutual Instruction Society,

BY ITS PRESIDENT,

JONATHAN HUTCHINSON, F. R. S.

OCTOBER, 1883.

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WISDOM AND KNOWLEDGE.



LADIES AND GENTLEMEN,

When I received, through your Secretaries, an invitation to become your President, I did not hesitate a moment as to accepting it. The compliment was, indeed, one which was very gratifying to me. I fear I must acknowledge that it is now nearly a quarter of a century since I was present at any of your meetings. It has happened, however, that the chief happiness of my life, during these long years, has had close association with an Essay Society on the one hand, and with Stoke Newington on the other. An invitation in which the two were combined came to me laden with happy memories.

Apart, however, from feelings which were personal, and which, perhaps, I ought scarcely to obtrude upon you, I think that I should have at once recognised, on public grounds, the claim of such a Society as yours to have its requests complied with. The Essay and Discussion Societies of our country, play a very important part in the moulding of its character and the making of its history. Yours has, I believe, been exceptionally successful. Founded by one whose sympathies with social progress were only equalled by his varied attainments in science, and in the midst of a community always remarkable for its zeal in education, it is not surprising that the Stoke Newington Mutual Instruction Society should have had a long and vigorous life, and

that now, at the end of five and thirty years, it should show no signs of age. How useful its career must have been we may in some degree infer from the list of Essays, &c. read before it, which your Committee has recently printed. Your laws have wisely excluded nothing from your debates but "party politics" and "sectarian religion," and various indeed have been the subjects which have been from time to time brought before you for critical examination.

The period during which your Society has been at its work has been by no means uneventful. It is possible indeed that future historians may regard the last half of the nineteenth century as an epoch distinguished alike by vigour of the human intellect, and by what usually goes with it, clearness of the human conscience. We have witnessed in it, I cannot but think, a very real increase in the sense of responsibility of the richer classes for those less favoured. Note, in reference to this, the spread of missionary effort, and its more liberal scope, as an agent of civilisation, as well as of theological teaching. Note the formation of schools, of the best kind, on terms accessible to all; note the wonderful development of cheap and pure literature; and lastly, the giant and very successful effort which has been made to reduce the misery which is caused by intemperance.

In each of these we may with gratitude recognise the formation of a social conscience leading to cheerful self-denial for the good of others, far, I believe, in advance of anything ever before known. If we turn to the achievements of the intellect proper, a picture of the same kind may be sketched. No age, excepting perhaps that of Elizabeth, has possessed at once such poets as Tennyson and Browning, such prose writers as Carlyle and George Eliot. In every department of Science splendid progress has also been made. Without the least fear of contradiction from any who understand the

subject, I may assert that Darwin's discoveries and reasonings have had for their results modifications of thought the importance of which to man's progress it is impossible as yet to calculate, and for which we might fail to find a parallel in the history of our race.

Permit me for a few moments to enquire as to what may have been the special function of our Society in the midst of all this vigour of intellectual activity? I think we may say of it, as of a multitude of others having like objects, that its work has rather been in the development of opinion, and the correction of error, than in the collection of new facts. Familiarity with the knowledge already in our possession, what may be called expressively *book-knowledge*, is to be obtained at schools, or in college-classes or from private reading, and the task of making fresh additions to its stock is undertaken by various special Societies, each of which restricts itself, more or less closely, to its own class of facts. None of our Scientific Societies make any attempt to go beyond their particular scope, or to do more than to collect and sift special facts. The vocation of literature and of Literary Societies is, however, much wider. It deals with the mutual relationships of different groups of acknowledged facts, and especially with their application to the formation of opinion and the enlargement of the grasp of the mind. Do I make a too ambitious assertion if I say that the main function of a Debating and Literary Society is to *develop knowledge into wisdom*? It is only by very slow degrees that the social opinions which constitute popular wisdom are formed, and when once formed it is by still slower ones that they are changed. They are based, speaking broadly, upon the sum of the nation's knowledge, and they have been developed, elaborated, and corrected by constant discussion. In this work daily conversation takes the largest share; the reading of newspapers, journals, and books comes

next; but the public and systematised form of discourse which we name *debate* is not far behind them in value. Not unfrequently Societies like ours add the word *Philosophical* to their designation, thus openly professing to be founded for the cultivation of *wisdom*. A *Literary and Philosophical Society* is a common form of designation, but one, I would submit, open to some criticism as verging on tautology, since, if we consider it well, the final end of all sound literature is the pursuit of wisdom. It is the business of literature to build for wisdom a house in which to live, to give it a body in which to walk abroad, clothes for its protection, and fitting adornments for its natural grace. It is a means to an end, and has in itself no final aim. It is possible that I may seem, to some, to use the word wisdom in a lower sense than that to which they have been accustomed. It is one of those words which come to us so surrounded as it were by preternatural halo, that we are apt to forget how simple and familiar are the things which they designate. This mistake is often a source of hindrance to us. We approach the contemplation of wisdom with an awe which enfeebles our knees and wraps our heads in cloud. We forget that, after all, this faculty, so sublime in its higher development, is one which is in degree universal, which the sage shares with the peasant, the elephant and the dog with the timid rabbit and the silly sheep. Especially, I think, do we make mistakes as to the sources of wisdom,—its genesis,—and in particular its relationship to what we call knowledge. Two of our most prized modern poets have, indeed, stated their creed on this point with remarkable emphasis. In each instance that creed is, I must contend, to a large extent an error. I refer to Cowper and Tennyson, and in each instance I do not doubt that the passage in question is well known to most of those whom I address.

I shrink from the attempt to give a definition of

wisdom, and yet for the purposes of our argument it is absolutely necessary in some manner to do so. Let it be suggested, then, that wisdom is that quality, or attribute, or attainment of the individual, which fits him for living well, and enables him to secure for himself and others, now and hereafter, the greatest amount of happiness which human life affords. It is a quality or attribute of the mind, not a something superadded to it. We can speak of wisdom in the abstract as we speak of sweetness, but not as we speak of sugar. If a man makes himself wise, in so doing he alters his mental constitution and becomes throughout his nature something different from what he was; he does not simply receive an addition. I can ask for sugar in my coffee in small or large quantity, or for its omission, and get it more or less according to my taste, but it is not so with wisdom. It, like the aroma of the coffee, is an essential quality of the thing itself, and can be increased only under the original conditions which gave birth to that thing. As the sun and rain to the growth and ripening of the coffee-berry, so are familiarity with Nature, and free intercourse with men, to the formation and ripening of wisdom.

It is necessary to clear our way by attempts at the definition of one or two other words. What is *knowledge*? what is *science*? what is *understanding*? By knowledge we will mean, if you please, the seeing of the truth—seeing, I mean, not so much with the outward as with the mental eye—the perception of the real facts as to things past and present, and the power to record and remember them. It is obvious that knowledge may be partial, limited, one-sided, and that the perception of one truth may be so mixed up with, and thwarted by, ignorance of another, that the resulting opinion may come to partake most of the latter element. Hence many mistakes result, and superficial observers may lay

upon knowledge the blame really due to its absence. Let us carefully guard against such blunders. *Science* is simply another name for knowledge, — the Latin name,—but it has come to be applied frequently to special branches of knowledge in which exactitude is desirable and possible. By *understanding* we mean the mind's power of appreciating knowledge, of distinguishing between the true and the false. The power of acquiring knowledge is, obviously, always in relation to the vigour of the understanding.

I hope that I yield to few in my reverence for all true Poets, and in my love for the two whose opinions, as to the relations between knowledge and wisdom, I am now about to bring under your notice, and not without some criticism. I trust that I love Plato, but I love truth more, and believing that both Cowper and Tennyson are in the wrong I am bound to say so. I would willingly have avoided an ungracious task, and at the same time the risk of failure in it, but that I feel sure that the error I shall try to expose is a very wide-spread and a very injurious one. It is one, too, which seems to have gained in force, if it has not even originated, in these latter days. A distrust of knowledge has sprung up amongst us, and a desire to exalt wisdom as if it were a something independent and almost antagonistic. I might quote largely from our modern sentimental moralists in proof that they have often written in oblique praise of ignorance. Thus we find George Herbert, tender, witty, and often wise as he was, writing "A handful of good works is worth a bushel of knowledge." As well might he have said that a peck of apples is worth an orchard of apple trees. Again, "Knowledge is folly except grace guide it"; true enough, but is not grace the crowning flower of the knowledge of good things? Show me the grace which is the offspring of stupidity or ignorance.

Cowper's poetry is full of disparagement of what he would call "human" knowledge. I will make, for our present purpose, but one quotation. It is from the 'Winter Walk at Noon':—

"Knowledge and Wisdom, far from being one,
Have ofttimes no connexion. Knowledge dwells
In heads replete with thoughts of other men;
Wisdom in minds attentive to their own."

I make bold to suggest that wisdom and knowledge have invariably the direct connexion of cause and effect, and that wisdom is simply the ripe fruit of the tree of knowledge. As a minor point, further, it is surely not true that knowledge is always obtained second-hand, nor that all wisdom is self-originated. The seeker after wisdom of the highest kind will often, perhaps usually, attain his object much quicker by the aid of a friend or of a book, than by solitary introspection. The latter process profits only those whose minds are already well stored with facts. If it were not so, where would be the value of education? Let me illustrate my meaning:—A kitten catching flies is guilty of the un-wisdom of seizing a wasp, and gets stung in the mouth. Had it known that flies were indigestible, it would have abstained from catching them; had it known that wasps could sting, it would have shunned most carefully the still greater error of treating them as if they were flies. Defining wisdom to be the state of mind which prompts to the line of conduct most certain to secure happiness, we have here a good example in proof that it has its root in the knowledge of facts. I give you here an instance of negative wisdom, or wisdom in avoidance of ill, but exactly the same argument will hold good as to wisdom in activity. We cannot judge between the causes of rejoicing and the causes of suffering, choose the good and avoid the evil, unless we know how to discriminate them.

Yet in spite of the criticisms which I have ventured, and which are probably obvious to most, there is a semblance of truth about what Cowper has written, and our minds revert to an impression that after all he was to a large extent right. Surely, it will be argued, we do not find the ability to live well and wisely always in proportion to the amount of instruction received, or to the familiarity of the mind with facts. This is certainly so, and nothing can be more true than that knowledge does not, immediately and directly, constitute wisdom. It must be developed, elaborated, become organised as it were, before it can assume the higher character. It was cleverly said of some one that "he had a great appetite for knowledge, but a very poor digestion," and of such a man it would certainly be true that he might load his mind with facts, but could never advance much in wisdom.

I just now claimed for our Society that its chief function was to help in the digestion of knowledge and its development into wisdom, and it was this assertion which led me into the present disquisition. This development, let us never forget, is necessarily very slow. All good things are, as the Greek has told us, hard; and can we expect that this, the greatest good of all, should be found an easy or a rapid process? The life-time of an individual counts for but little in the elaboration of wisdom, and, for the most part, what we name as such has been the heirloom of many, very many generations. Not the less did it take its beginning, and receive its increments, from the acquisition of knowledge. It has been said that it takes three generations to breed a gentleman, and at least three to acquire the gout, but the growth of wisdom is far slower. In proof of this let us remember the remarkable sameness, as regards this quality, through many successive generations of the same races. As far as wisdom is concerned the

Celt remains a Celt, the Jew a Jew, and the Negro a Negro, through the longest periods of historic time. There is progress, I doubt not, but it is in most races, and in most times, so slow as to be almost imperceptible.

The quotation which I have made from Cowper's Task is completed by the following lines :—

“ Knowledge, a rude unprofitable mass,
The mere materials with which wisdom builds,
Till smoothed and squared and fitted to its place,
Does but enumber whom it seems to enrich.
Knowledge is proud that he has learned so much ;
Wisdom is humble that he knows no more.”

Is it not the fact that in these plausible statements the poet is merely applying the term wisdom to old knowledge, and drawing such distinction as might well be made between an apple just formed in August, and the same fruit when ripe in October ? Knowledge forms the materials not with which wisdom builds, but out of which wisdom grows. The bricks ranged in ungainly heaps, fresh from the kiln, may be compared to knowledge ; the same bricks when built into a house for protection, comfort, and ornament, may be compared to wisdom, the artificer being the understanding. Bricks and a house are not the same thing, but we cannot have the house without the bricks.

I will next read you a quotation from Tennyson :—

“ Who loves not knowledge ? Who shall rail
Against her beauty ? May she mix
With men and prosper. Who shall fix
Her pillars ? Let her work prevail.

But on her forehead sits a fire :
She sets her forward countenance
And leaps into the future chance,
Submitting all things to desire.

Half-grown as yet, a child, and vain—
She cannot fight the fear of death.
What is she, cut from love and faith,
But some wild Pallas from the brain

Of Demons? fiery-hot to burst
All barriers in her onward race
For power. Let her know her place;
She is the second, not the first.

A higher hand must make her mild,
If all be not in vain; and guide
Her footsteps, moving side by side
With wisdom, like the younger child:

For she is earthly of the mind,
But Wisdom heavenly of the soul."

* * * *

It is impossible to read these grand lines without admiration and without sympathy with their general drift. Yet they are, I cannot but think, false in their metaphysics, and untrustworthy in their teaching. Is it possible to cut love and faith away from knowledge and to find them a basis on its absence? A knowledge or perception of the true and the beautiful is surely the only foundation for reasonable love. In truth, so inseparably are these connected, that love might almost be described as being the sentiment produced by the perception of beauty. If faith is to be anything more than the merest superstition, it must find its basis somewhere in knowledge. From what we know as to the past and the present we infer as to the future, and this inference we call faith. In proportion as we can trust our knowledge is our faith strong or weak. It is obviously impossible for anyone who knows himself wholly ignorant of the future, to feel any strong faith respecting it.

Again, when our poet denies that knowledge can fight the fear of death, I cannot but think that he is mistaken.

I am aware that I am approaching a very serious subject, but must yet ask to be allowed to say a few words upon it, for it is one which is far too often avoided.

The dread of the cessation of existence, the loss of the personal consciousness which we term our life, is so deep-rooted in all, so absolutely instinctive, that its conquest may be well regarded as a test of the value of any mental acquirement. Tennyson, in the lines which I have quoted, records his conviction that this conquest cannot be made by knowledge. The dread of death is felt, let us note, in full intensity, by all animals, and it is the source of almost all the conditions which we mean by the term terror. Perhaps it might be safe to assert, within certain limits and with exceptions here and there, that this dread diminishes with education and becomes less abject as we advance in civilisation. Bacon, in his remarkable Essay on this subject, reminds us how easily it is overcome by many of the emotions and passions, asserting that "there is no passion in the mind of man so weak but it mates and masters the fear of death." He further remarks that "revenge triumphs over death, love slights it, honour aspireth to it, and grief flies to it." He would appear to have himself entertained remarkably little of this fear, which he speaks of as "weak," reminding us that "it is as natural to die as to be born." Finally he assures us that to a mind "fixed and bent upon somewhat that is good" "the doleours of death are averted," and in very remarkable words adds, "but, above all, believe it, the sweetest canticle is, 'Nunc dimittis,' when a man hath obtained worthy ends and expectations."

Shakespeare, as we all know, makes the dread of death to consist in our ignorance of the hereafter, "the undiscovered country." Let us keep a clear distinction between reluctance to die and fear of death. The one is a healthy sentiment which it is not to be desired that

knowledge, or any other acquisition, should ever displace. Under all ordinary conditions of health and happiness we joyfully acknowledge the duty of delight in life, and we carefully seek to cherish a feeling of unwillingness to end prematurely our association with the beautiful world into which we have been born. The fear of death is something quite different from the cessation of zest for life, and it is as to the effect of increase of knowledge upon this fear that we have to enquire. Let us remark in passing that if advance in true knowledge cannot help us, it is yet quite certain that suppositious knowledge can. The Mahommedan believes that he knows that there is for him a secure abode of bliss after death, and this creed, which is to him the equivalent of knowledge, entirely overcomes all dread of the change. It were strange, and most pitiable, if true knowledge in this matter were less comforting than that which we believe to be a mistake. Let us try to see how the matter stands, or rather let me briefly glance at one or two topics in connexion with it, for there are some of great importance to which it would not be fitting that I should even allude on the present occasion.

Is it not the fact that our conceptions as to the order of the universe, and the nature of the great Spirit in whom we live, and move, and have our being, have, from primitive times onwards to the present, been purified, dignified, and exalted? I refer not only to times of paganism, but those of Roman Christianity, and to yet more recent times. One by one superstitions more or less gross have become impossible and been put aside, and love and trust have year by year found firmer ground in the belief that there is neither variableness nor shadow of turning in that glorious order of things in which we are called to play our part. In that order Death, the cessation of individual existence and the transference of life and life's duties to others, is an essential part. To this pro-

gress in opinion the study of history and of science have been mainly contributory. They have further shewn, in the most conclusive manner, that death is really no interruption to life, but almost synonymous with its renewal. Although personality or individualism be but temporary, life, we begin to perceive, is permanent and immortal. The grand old Hebrew metaphors come over us with gain, both in beauty and truth, from our familiarity with modern knowledge. We all do fade as do the leaves, but we are renewed as are the leaves, and no single leaf, however short its life, but has contributed its share to the glory of the tree. Amongst the foremost of the results of modern science in its strictest sense (of examination into the laws of heat, electricity, and the like), is the perception of the indestructibility of all forms of force. The famous line which Shelley applied to the cloud, so fleeting in form, yet so immortal in being,

“I change, but I cannot die,”

is found to be literally true of all nature, not only of the physical world but also of the moral. I cannot but believe that all who gain a perception of this doctrine, secure for themselves a great increase of trust and comfort in life. It becomes possible in this creed to lead quiet, hopeful, unfevered lives. If we be but true there is no need for hurry, the forces of nature are all with us, and however brief the lives of individual men, “Man has for-ever.” Working in this spirit, each life, long or short, is complete in itself, and every year, every week, every day, is sure of its result. It is as impossible that anything can be lost, as that any atom of matter can be destroyed. I claim that these sentiments are the legitimate offspring of modern knowledge, and I cannot but think that it is possible for the mind which is imbued with them to attain to something very near to victory over the fear of Death.

The sense of continuity becomes the perception of immortality.

The distrust of knowledge in reference to moral gain, and the belief that it is possible to attain to wisdom without its aid, which we have noted, are, I think, mainly of modern growth. The wise Hebrew King, in those beautiful chapters which introduce the book of Proverbs, makes no such distinction, and is as warm in the praise of knowledge as he is of wisdom, treating them almost as if they were the same thing, or at any rate as if the one were the means to the other.

“To know wisdom and *instruction*; to perceive the words of understanding.”

“To give subtilty to the simple, to the young man *knowledge* and discretion.”

“A wise man will hear, and will increase *learning*.” “Fools despise wisdom and *instruction*.”

“How long, ye simple ones, will ye love simplicity? and the scorers delight in their scorning, and fools hate *knowledge*?”

“Yea, if thou criest after *knowledge*, and liftest up thy voice for understanding.”

“Happy is the man that findeth wisdom, and the man that getteth *understanding*.”

Everywhere the fool (i. e. the non-wise) is spoken of as a despiser of knowledge.

Bacon's eloquent praise of knowledge is known to everyone. He clearly had no misgiving. It is the same with Shakespeare; and although he puts the words as a kind of chance expression into the mouth of a Jester, I suspect that he summed up his own belief when he declared, “there is no darkness but ignorance.”

Let me also remind you of a line of Young's, often quoted but possibly more praised than pondered.

“The undevout astronomer is mad.”

Let us analyse this line and see to what conclusions it will lead us. To be mad is to have the mind in such a

condition that it fails in an egregious manner to perceive the true relations of the facts which it knows. An astronomer is one possessing a good knowledge of the facts as regards the countless worlds which occupy space. To be "undevout" is to be without sense of devotion to the maker of all things, the Spirit of the universe,—to feel as if personal and self-existent, and owing no reverence to the unseen. It is clear, then, that in Young's opinion the natural result of physical knowledge in this department is gain to the devotional or reverential spirit. I cannot doubt for a moment that he is right, and will assert further that the revelations of the telescope do not differ in their tendency in this respect from those of the microscope or the scalpel. Advance in physical knowledge may rid us of superstitions, but will never do otherwise than help true faith and tend to devotedness of life.

In what respect, let us ask, is it correct to speak of knowledge as a "child," "half-grown"; in relation to wisdom, "a younger child"? Simply, so far as I can see, in this, that we have, always extant, a certain amount of newly-gained knowledge as yet unapplied and immature. This must always be the case, and if we choose to call old and ripened knowledge by the name of wisdom, then the expressions I have quoted in reference to that which is young will become appropriate. Not the less, however, does it remain the fact that in the first, and in every successive stage, the acquisition of knowledge preceded the formation of wisdom. Knowledge may in some sense be said to be the perception of truth in fragments, whilst wisdom takes cognisance of it as a whole; or possibly we sometimes use the words as if knowledge meant the perception of truth in minor matters, and wisdom in those of first importance. Such definitions are, however, clearly only conventional, and make no reference to the real relationships of the two. In a far deeper

sense we might say of wisdom that it is human instinct applied to the conduct of life. This would lead us to ask how instincts arise. Granted that they are seemingly intuitive in the individual, being born with him, and transmitted by him to his children, we shall yet be obliged to admit that in his remote predecessors they began in the acquisition of knowledge. They are the crystallised sum of knowledge; they are knowledge ingrained by hereditary transmission. They bear perhaps in extreme instances almost the same relation to young knowledge that the diamond does to carbon. Yet, although so different, there is still the relationship of parent and child. The bird did not learn to build its nest, nor the bee to form its comb, all at a jump. In each case there were at first trials and partial failures, then finally success and a habit formed which became capable of hereditary transmission. Such, I believe, are the opinions of all who have, in the light of modern facts, examined the facts as to instinct in the lower animals. Precisely the same reasoning is doubtless applicable to those higher instincts in man which we name morality and wisdom.

I am aware that to some it will appear to be an objection to my argument that we do not always find the possessors of knowledge capable of nobleness of life. Is there not, it will be asked, after all, a faculty distinct from knowledge, and superior to it, which fits its possessor for self-denial in the path of duty? We must not confuse knowledge or even wisdom with conscientiousness or strength of will. These depend upon other conditions of mental organisation, and may unquestionably be present in high degree without any recent additions of knowledge. They, like wisdom, are matters of organisation and of hereditary transmission. We may define conscientiousness as the sense of responsibility for doing what we believe to be right, and this sense unquestionably

varies much in different persons. Given, however, two individuals in whom conscience and will are equally strong, it is clear that the advantage would rest with him who had wisdom and knowledge in addition. As a matter of fact, indeed, conscientiousness without knowledge and wisdom is often a most inconvenient feature of human character, and so also is strength of will. What we want is to develop sense of responsibility side by side with the development of knowledge.

It is time that I should conclude. If I have troubled you too far with definitions and explanations, I must remind you that my motive has been good. I have been aiming all the time to strengthen the hands of your Secretaries and those who take the chief share in the work of your Society. I have been trying to convince all of the great value of your meetings as a means of advancing that greatest of all life's aims, the increase of Wisdom. Rest assured that you will gain this result. Never distrust knowledge, for it is the only path to the desired goal. Be fearless in its pursuit, and let your fearlessness be the result of sound faith. Keep aside, according to your excellent rule, *Sectarian* religion and *Party* politics, for they lead to quarrels; but with these exceptions debate boldly on everything else. Try to look at all sides of things, and know that this is the only way to truth. There is no road to wisdom but through knowledge, therefore let us love knowledge.

I purposely omitted in their place the lines with which Tennyson concludes. Let me now give them.

* * * *

"O, friend, who camest to thy goal
So early, leaving me behind,

I would the great world grew like thee,
Who grewest not alone in power
And knowledge, but by year and hour
In reverence and in charity."

As we all know, 'In Memoriam' was addressed to the memory of a buried friend. Concerning this friend the poet had no misgiving as to increase of Knowledge having led in him to increase in Wisdom and in Love. What was wanted was the faith to believe that this was the natural and universal result. We are all of us apt to think that what we call the "world," at a distance from us, proceeds on different lines from those attained to by ourselves and our immediate friends. Let us seek more faith, and try to realise that this impression, so universal but so illusory, proceeds only from want of strength in the power of realising what we do not see. Could we see clearly we should know that the tendency of the human spirit is as constantly and inevitably upwards as that of trees. All forms of life seek the sun and the light. Let us thankfully trust the "Great World."

AN ADDRESS

DELIVERED AT

THE CONVERSAZIONE

HELD AT THE

LONDON HOSPITAL

OCTOBER 2ND, 1882.

BY

JONATHAN HUTCHINSON, F.R.S.,

SENIOR SURGEON TO THE HOSPITAL.

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AN ADDRESS

MR. CHAIRMAN, LADIES AND GENTLEMEN,—

I HAVE been asked to say a few words this evening, and the choice of topic has been left to me. In some respects I should have much preferred that it had been given me, and that Mr. Buxton, Dr. Clark, or some one of the many friends whom I see around me, had chosen my subject, and thus diminished a somewhat oppressive sense of responsibility.

There is one topic which comes to the mind almost naturally when teachers and students meet again after a holiday, and that is, to ask how the vacation has been spent. I do not, however, propose, gentlemen, in any inquisitorial manner to ask what holiday tasks have been performed or how far the good intentions of July find themselves fulfilled in October, whether or not you have really read the books which you had determined to read. If for no other reason I am deterred from venturing in this direction by the character of my audience, for we have the pleasure of the company of many who are not students of medicine. I propose, therefore, on this account, to discard professional topics, and making my own guesses as to the literary pursuits of the past two months, shall try if I can find in connection with them the material for half an hour's comments.

I will guess, then, that we have most of us recently been

brought into more or less sympathy with Mr. Paul Bultitude under the painful circumstances of his transformation as told us in "Vice Versa," and that we have almost all of us read with a kind of pleasure, of which I hope we felt a little ashamed, the clever but exaggerated caricature which has appeared under the title of "Democracy." Concerning neither of these shall I have another word to say; my real topic will be a work of no such ephemeral interest, but one which will hold its place amongst our literary treasures for many a year to come. We are, I doubt not, most of us fresh from the perusal of the 'Life of Thomas Carlyle.' About a year ago there appeared, immediately after the death of one who was perhaps, take him for all in all, the greatest man of the present generation, two volumes of Reminiscences. In these we found, amongst much that we valued, much that needed excuse, and some things that it was hard to forgive. Those, however, who were most pained by certain passages in that work will, I cannot but think, have been greatly relieved by the reading of the two volumes of biography which Mr. Froude has just given us. In these latter we have, full and complete, the story of Carlyle's life, his struggle, and his victory, and the hero now again claims, if I mistake not, that lofty position in our estimation from which he had been almost threatened with dethronement. Here we are permitted to see, in full disclosure, the sources alike of his weakness and his strength, and our marvel at the work which he did, and at the uses to which he put his troubled life, rises higher than ever. The unconquerable independence, the industry, the faith in the future, unclouded in the main by the deep-rooted melancholy of his nature, the sense of duty in his work, are features in which the character of Carlyle attains sublimity. His letters to his wife and to his mother, and perhaps above all those to his brother John, claim our admiration in the very

highest degree. Especially to all young men, and to all young medical men, for they were addressed to a struggling young physician, I would commend his letters to his brother. His letters to his mother make constant appeal to the difficulty under which he feels himself in using expressions which she will understand and which will not give her pain. She had remained a Scotch Puritan of the most austere creed, and to her no expressions on religious subjects were acceptable, scarcely, indeed, intelligible, which did not clothe themselves in the phraseology of the Hebrew Scriptures. To her son many of those expressions had lost their fulness of meaning and seemed to need new translation in accordance with the wider scope of modern thought. Over and over again he has to assure his poor mother that she and he, although they phrase it differently, really mean the same thing and have their hearts set on the same objects,—an explanation and assurance which in the present day many thousand sons find themselves I fear constantly compelled to make.

In truth, it was not an inconsiderable demand upon a middle-aged matron of strict opinions, to expect that she should believe that 'Sartor Resartus' really meant nothing more than what she had been taught in her early life. The book is itself a protest against the enslaving nature of dogma, and an enforcement of the duty of thinking, and above all of feeling, for oneself. It abounds in strange and out-of-the-way expressions, and that not because its writer loved singularity, but because his main mission was to try to break the bonds of formula, and make the reader see that there was a substance behind, which current and well-worn phrases often tended rather to conceal than to show. As its name imports it was an attempt to re-clothe human opinions in fresh and more vivid modes of expression. As the man is more than the garments which he wears, so it asserted, is the very thought itself more than the phrase in which it is

eustomary to array it. Let us put the thought into a new dress, let us, if need be, strip it naked, and see then how it looks. But this process, however useful, however necessary, to a large majority is not only a difficult one, but is also attended with pain. An old and well-known friend seems changed, and changed too often for the worse, by being made to wear a new-fashioned dress. For Carlyle's purpose the method he adopted was absolutely necessary, and what is more it attained its end. Many before him had recognised the exceeding importance of precision in language, and had seen clearly enough the truth which Hooker so well expressed when he said that "the mixture of those things by speech which by nature are divided is the mother of all error." It was more than this which Carlyle saw; it was that in all forms of language, and especially in all current phrases, there is danger—a risk that the thought—which should be living and free—may become, as it were, imprisoned, and no longer capable of expansive growth. Is it too much to say that with the publication of 'Sartor Resartus' there came in a new order of literature, and one vastly superior to that which it superseded? Critics, essayists, biographers, and historians, one and all began from that time to see things in a new light, to try to discriminate better between the husk and the kernel, to place a far higher value upon fact, and to spare no pains to make their words convey with vividness their meaning. In this way the book itself has been half superseded, for all the leading writers of the day have caught some share of its lessons. I am old enough myself to remember the day when this book was at the height of its popularity, and when village ladies and young gentlemen formed themselves into classes for its study, with the design of reading a few pages every day, and the not unnecessary pledge that there should be no going forward until what had been read was well understood.

I have wandered, I fear, rather far from my special topic, which was, I believe, to explain to you how it came to pass that Carlyle's mother, herself the wife of an Annandale mason, found some difficulty in understanding the writings of her eldest son, and was anxious and suspicious as to their strict orthodoxy. Perhaps after all it was hardly necessary to explain it.

Over and over again, in his letters to his brother and in his entries in his own diary, Carlyle enforces the paramount duty of keeping clear of the "gig-man" spirit. In one of his most pathetic letters to his wife, when their prospects were at the lowest, and he is endeavouring to incite her fortitude to further effort, he reminds her with pride that her soul was never that of a "gig-maness." To many present it is, I know, not necessary that these terms should be explained, but it may be that there are some to whom elucidation is desirable. Especially perhaps is it possible that some of the young ladies before me may not feel quite sure as to in what consists the sin and shame of being a gig-maness. Since it would be a great pity that any one should run the risk of becoming a gig-maness unwittingly, or should be incapable of understanding the compliment if told that she is not one, I shall venture to offer a few words of explanation. The words in question are designed to denote merit and respectability as indicated by externals alone and without regard to the inner character of the man. It is said that at a celebrated criminal trial a witness deposed that he had always considered Mr. Thurtell a very respectable man, and when asked why he had thought so, replied, "Well, he kept a gig." I have heard other forms of the same anecdote, all turning upon the point that the keeping of a gig in the times when gigs were more in use than now, was an accepted sign of a certain social position. Hence, to be a gig-man, in Carlyle's meaning of the word, is to trust to externals as

our credentials to the respect whether of ourselves or of others. To be above the gig-man spirit is to be capable of maintaining our proper position in the forum of our own feelings irrespective of success in life, and to be in the habit of according such position to others quite irrespective of theirs. It is to take measure of the soul rather than of the body's trappings. In proportion as I permit to myself no sentiment of inward satisfaction excepting in regard to things pertaining to the soul's growth, do I keep clear of the gig-man standard. If I find my happiness, on the contrary, in increase of wealth, or the acquisition of titles, then I am a gig-man. The social laws which draw abrupt lines of distinction between first-class and second-class passengers, between members of professions, and those who follow trades, are laws of the gig-man order. If a young lady in her preference of one admirer to another allows herself to be influenced by the consideration that one is a doctor and the other only a draper, knowing all the time that the draper has the warmer feelings and the sounder taste, then I much fear that she is a gig-maness.

There is yet another word frequently occurring in these letters which it may be necessary to explain. It is the German word *entsagen*. Carlyle repeatedly congratulates his brother on having learnt the meaning of this word, and in one place he writes "My main comfort about you is to see the grand practical lesson of *Entsagen* impressing itself in ineffaceable devoutness on your heart." He regards *entsagen*, he says, as the first lesson in all true life. With Mr. Froude's help we may interpret his meaning to be that we should in the first place learn the doctrine of renunciation of worldly gains and become able to say firmly that we can do without any and all of the various pleasant things with which the world usually rewards those who render services to it. It is, in fact, to learn, as we find

it expressed in 'Sartor Resartus' "to do without happiness and to find in its stead blessedness." Without doubt a most important attainment. Respecting, however, both this doctrine of *entsagen* and that of gigmanism, I feel inclined on behalf of the weakness of human nature to protest against their being pushed too far. They are, if so pushed, the virtues of the stoic and fit only for the few. The world would lose much useful work if ready-money wages in current coin should cease to be paid or should lose their present value. We are all of us gig-men more or less, and there are times when we all find it helpful to look forward to early and common-place forms of reward. The donkey that was enticed onwards by a bunch of carrots fixed on the end of the cart-shaft at any rate accomplished his journey and probably got the carrots at last, and, however humiliating it may be to say so, he is in truth but an illustration of the mode in which much of the labour of the world is exacted from those of us who have to toil long after the zest for work is over. *Entsagen* for the most part is, let me further say, a doctrine for the grown-up man, and should not be enforced either on the young or on those who have the care of them. Many, too, beyond the age of childhood, find it needful to cultivate the amenities and delights of life lest too great austerity should land them in mental disease.

Were we to attempt to sum up in brief the secret of Carlyle's strength, I think we should have to say that it consisted in his perception of the reality of things. This was a feeling which was ever present with him. Men were real to him, spirit-possessing beings, never wholly without the capability of affection, never even in the degraded conditions of partridge-shooters or gig-men losing all share in the divine element of life. Their differences, too, were real and must be taken carefully into account. The world was real, the universe real, the past had really been, and the future would most

certainly come. In truth, this sort of perception is the secret of strength in us all, and its absence is the cause of all weakness. It is the very basis of all motive and of all effort. It measures our devotion to truth and our belief in its value, upon it is rooted the distrust of all shams, the hatred of all forms of lying.

The gospel of duty, self-restraint, and devotion to work, was one which Carlyle had well learned, perhaps no man ever better. But there was a great failing in his attainments, one which marred the happiness of his life, and which not only robbed him of the reward which was his due, but considerably diminished the usefulness of his teaching. He had not learned what we may, I think, without irreverence style the Religion of Patience. By patience I mean not the mere passive virtue of endurance, which indeed is not unfrequently no virtue, I mean rather the ability, when we have done our best, under all possible circumstances to rest undesperingly and trustfully for the result. Dare I venture for one moment to assume the prophet's mantle, I would foretell that the worship of Patience in this exalted sense is one upon which the present age is about to enter. It is one to which all the scientific studies of modern times point us. Alike in geology, the history of animal life, and in that of the progress of human society, we are presented with lessons which teach us that creations and cataclasms are rare, and that we must put our confidence in "the long result of time." If we glance back over the great mythologies of the past and note their hidden meanings, we shall observe the worship under various types, of various forms of power, of beauty, and of virtue. In the earliest ages the gods were symbols of force, they did, and that not always beneficently, the deeds which controlled the destinies of men. Next, as in such types as that of Hercules, we see force combined with

human-heartedness, but still force, gross and almost purely physical in its efforts. Side by side with this sprang up the worship of beauty, especially in human and female forms, and the shrines of Juno, Minerva, and Venus, under various names in different climes, claimed their countless votaries. As the moral sense grew and human sympathy expanded, the unsatisfying nature of these Religions of the External became felt and the world witnessed events such as the self-renunciation of Buddha and the advent of what has been well termed the Worship of Sorrow. In these sublimely loving creeds there were, however, elements of weakness and of unfitness for the everyday work of the world, and the pendulum of human sentiment, as it was sure to do, swung back again towards an exaggerated estimate of physical force and natural beauty. In truth, the worship of these was far too deeply rooted in our very being for it to have ever been put aside. Another had been added, but these had not been dethroned. Nor will the introduction of a new goddess effect the displacement of any one of her predecessors. We may pay our vows at the altar of patience without ceasing also to render due homage to courage, energy, and physical vigour, and without bating one jot of our admiration for the charms of external beauty or of our reverence for that glory of soul which can place the happiness of life in doing good to others. I am aware that in offering these suggestions I am giving but the barest outline of a subject full of detail and complexity. But we may probably still find that it is not very far from the truth, and that in all ages men have given their reverence, if we put aside their fears, first to manifestations of power, next to those of beauty, and lastly to self-abnegation. In the temple which encloses these three shrines we must all still worship. If we cease to revere strength we shall reap as our reward weakness, if we shut our eyes to beauty we shall

lose the joyousness and brightness of life, if we fail to feel the divine attractiveness of self-abnegation we shall soon find that the priceless capability of sympathy and love has faded from our hearts. We may thankfully recognise that there is at present no sign that the world is likely to fail in its allegiance of to any one of these three, and the sooner that the shrine of Patience is admitted as a fourth the better will it be for us all. I have let slip an expression in suggesting that Patience should be typified as a goddess, which I ought perhaps to recall or qualify; for it may be doubted whether the symbol of a female god would be quite appropriate. What is meant by the patience which the world is now in need of learning, and respecting which Carlyle so definitely failed, is the power on all subjects to receive all facts without prejudice, to accept the work, imperfect though it may appear, which is done by others, to be hopeful and trustful under all circumstances, to bear our lot in life, when unalterable, without resistance and without complaint. Other things being equal, patience gives, to the character which possesses it, an enormous advantage, for it shields the mind from a thousand sources of turmoil and discouragement. Here let me say that if Carlyle failed in the exercise of this virtue he did not do so in his appreciation of it. The German motto "Ohne Hast ohne Rast"—without hurry and without rest—was constantly on his tongue; and many and forcible were his exhortations to his brother to take courage, and vigorous were his expressions of faith in the future of the world. But in spite of this his patience often broke down and he formed harsh judgments of others, both of their actions and opinions, simply because he could not compel his mind to examine them with candour. Part of his impatience was hereditary, being due to the greatness of his nature in other directions, his keen appreciation of the true making him correspondingly intolerant of what seemed to him false.

This topic of Life-patience is one which concerns us as medical men, perhaps, more directly than some of the others which I have mentioned. To a large extent, impatience of life in its various forms—acute, chronic, and paroxysmal—is undoubtedly a result of inherited organisation or of derangement of health. Its cure, if cure there can be, must be sought from physical means and not from any new development of opinion or fresh insight into the order of the universe. That, however, the influence of opinion and of creed upon the mental health is often very great, no one knows better than the medical observer. This evening I purpose to eschew all purely medical matters, and I shall therefore now confine myself to a few remarks upon the bearing of different modes of belief upon cheerfulness in life. It is needless to remark upon the absurdity of exhorting a man to be cheerful or to be patient. You might just as reasonably exhort him to be six feet when he is really only five feet ten. The problem is to make him cheerful, and it cannot be done by preaching to him concerning the duty. Now, in studying the genesis of patience I think it may, in the first place, be admitted that patience is in the main based upon hope; “We live by hope, we breathe the glad air of a bright futurity, and so we live or else we have no life.”

Next let me assert that it is not so much the greatness of the things we hope for as the certainty that they will come which makes us feel able to wait. We are not by nature gamblers, loving to stake our all upon a throw, and the prospect of small but certain profits has a far more attractive power over most minds. I have said that Carlyle was impatient, and that he formed, under the influence of prejudice, very unjust opinions on some topics. Amongst others, he spoke of the doctrines of Darwin as too contemptible to be worth a moment's consideration. In putting them thus scornfully aside, I think he missed a main source of comfort

in life. The truth is, that what Carlyle himself was proclaiming in the language of the mystic, Charles Darwin was explaining in the language of science. Carlyle was asserting that there is a spiritual power in nature, was bidding us reverence that power as supernatural, and as working through rough and mysterious ways towards certain and definite good. Darwin, looking at the same facts from a biologist's standpoint, explained how this result did indeed come about, and that, too, through the simplest and most unmysterious ways. It has been thought by many, by believers as well as sceptics, that Darwin's explanations are melancholy ones, and that they would in short land us again in regions of mere brute force. I cannot think that this view is correct. Darwin did not impose any new "law," he simply interpreted the facts of nature, and nature, whether his explanations be true or false, will go on in the future as it has done in the past. We may then expect confidently in the future the same kind, perhaps the same rate, of progress which there has been in the past. This consideration should certainly forestall despair, and there remains another which may, I think, reasonably give us a lively hope. When we use such phrases as "Survival of the fittest," "The battle of life," "The struggle for existence," and the like, we by no means have regard to brute force only; friendship is also a force, affection and love are forces of incalculable power, and it is with these which we have to reckon in estimating the prospects of the battle. That victory will in the long run be found on the side on which they are ranged is certain if we reflect for a moment on their nature. Love and friendship, as well as hatred and selfishness, will make use of material means and brute force in order to success. These means will be common to both, and since love leads to union and hatred to isolation, love must of necessity in the end prove the stronger. Surely a

mind familiar with such thoughts gains much in the solid foundation of its hopes.

There is nothing here erratic or uncertain. Under the laws which the Creator has given to life, simple as possible in themselves, there must be progress in earthly happiness. Nor do the facts of history belie this speculative conclusion. Did time permit, it would be easy from various other departments of natural science to produce arguments of a similar nature, and show how much knowledge in these directions tends to produce patient hopefulness as to the destinies of mankind.

I trust that I shall not hurt the feelings of any by using the term prophet as applicable to Carlyle. In truth there is none other suitable. Not one of the great Hebrew seers was more impressed with the importance of his message or more resolute to deliver it faithfully than he. None ever more profoundly revered the unseen Power, or sought more earnestly to live always as under his Master's eye. We must make allowance for changes in the age of the world, and for differences thus brought about in modes of expression, and these being made, we shall find in Carlyle by far the closest resemblance to such men as Jeremiah, Isaiah, and Ezekiel that the present age has produced. He foretold, not the fall of this or that city or kingdom, but the general prospects of his race; and he denounced, with a fervency at least equal to that of those whom I have named, the penalties which must come from want of truthfulness, want of honesty, or in a word from ceasing to fear God. Nor were his modes of expression and of publication so very different from those of old, and what differences there were he could not well have avoided. At one place we find him declaring, "Had I but two potatoes in the world and one true idea, I should hold it my duty to part with one to buy pen and paper and live upon the other till I got it written." At another he expressly states that the great

Hebrew writers were those with whom he most elosely sympathised, finding in them an earnestness akin to his own. Like them he had to bewail that his message found no hearing. "One's heart," he wrote, "is for hours and days overeast with the sad feeling: 'There is none, then, not one, that will believe in me!' Great in this life is the eom-munion of man with man. Meanwhile continue then to believe in *thyself*. Let the ehatattering of innumerable gigen pass by thee as what it is. Wait, thou, on the bounties of thy unseen Taskmaster, on the hests of thy inward *Dæmon*. Sow the seed-field of Time. What if thou see no fruit of it, another will. Be not weak. One way or other, thy message will and shall be uttered. Write it down on paper any way; speak it from thee—so shall thy painful, destitute existenee not have been in vain. Oh, in vain! Hadst thou, even thou, a message from the Eternal, and thou grudgest the travail of thy embassy! O! thou of little faith!" With many such expressions do we find him in his private diary striving to sustain and strengthen his sense of responsibility in his work. The message which he believed that he had eliefly to deliver was that the Natural is also the Supernatural. He sought not to degrade the supernatural by bringing it down to the natural, but rather to elevate the natural. In this there is doubtless a great meaning, but at the same time somewhat of the jargon of words. To say that the natural and the supernatural are really all one, and both the work of one and the same Maker, is probably to most of us the simplest and at the same time a suffieient mode of expression.

Many ages have had their prophets and their seers, those who in uttermost earnest have set themselves to deliver the messages with which they had been entrusted. If I am not mistaken, however, no age and no eountry has been more favoured in this respect than our own. I will name to you

four, not that they are the only ones but rather that they stand foremost, and because, also, I think they may be fitly held to represent the four divisions of social religion to which I have referred. To Carlyle, of course, we give the chief place, and to him we assign the priesthood of the worship of Strength. To him it was permitted to see that all power, physical, intellectual, and moral, is alike God-given and must be used for the Maker's ends. His message was "Be strong, and to that end be truthful, be honest, for in falsehood and dishonesty there cannot possibly be other than weakness. Reverence your strength." Of him I have already said all that time will allow.

There can be not the slightest doubt as to whom I ought to name as our Seer of the Beautiful. Note first how the domain of the Beautiful has of recent years been enlarged. No longer are our conceptions of it almost solely associated with human or at any rate animal forms, we now find delight in a thousand aspects of inanimate nature to which our forefathers were well nigh blind. Let us note, also, that only within the present century have we shaken ourselves clear of the trammels of an outworn Puritanism, which forbade us to seek pleasure from beauty of any kind. We do not owe our emancipation in this respect wholly to one man. There is, however, one author who, by his own intense purity of taste and almost unexampled power in the use of language, has done more to develop the perception of beauty in the minds of Englishmen than the rest of his generation taken together. Not only has he proclaimed "the duty of delight" but he has taught us how to practise it. What Carlyle has done for the worship of Strength, his pupil, John Ruskin, has done for that of Beauty.

To Wordsworth must, I think, be assigned the office of latter-day priest at the shrine of self-renunciation and human sympathy. The early-orphaned son of a country

attorney, he devoted himself almost from boyhood to the task of repressing the pride of wealth and power, and of showing us the loveable side of lowly life. Having first, as all prophets must, trained his own heart by observation, and cultivated his own feelings until he could trust them, he then, with a calm carelessness of criticism, and without hoping for applause, set himself to his task. He had known from his youth the details of the lives of the peasants of the dales, next in Cambridge, then in London, and, lastly, on the Continent he brought himself into close contact with other phases of human life. His was not thoughtless feeling, he desired to know how his sympathies should be best directed. In his "Prelude" he has told us, in simple and pathetic language, what his feelings were whilst mixing with the poor of London and of France. Making allowance for difference in time and country, I do not think that even Buddha himself, when he left his father's palace for the desert, manifested a more calmly self-renouncing spirit than did Wordsworth at this epoch of his life. He would live on the smallest pittance, in the least possible cottage, and there he would do his share in the world's regeneration. The age of hermits and devotees was happily passed away, had it not been so he would have become one. As it was he married a wife, brought up a family, did his best to provide things honest in the sight of all men, and through a long life worked on with a most noble singleness of aim. Wealth and honours never tempted him, and probably not in any subject that he selected, not in a single line that he ever wrote, was he influenced in the least by the desire of praise. He did not preach, he was not didactic, his power was far higher. Instead of telling us what we ought to be, he finds us the means of becoming; he does not instruct us, he cultivates our hearts. The "Prelude" and the "Excursion," although studiously calm and self-repressed, are full of

narratives which it is impossible to read without emotion or to dwell upon without profit to the soul. His "Happy Warrior" and "Ode to Duty" ought to be kept fresh in the memory of every young man.

His nature was, however, not strong on all sides. He was ignorant in science, especially in biology and political economy, and very distrustful of them. His sympathy with the exercise of natural strength was but limited, and I do not think I should be wrong in even saying the same of his perception of external beauty, unless it could be linked with some human lesson. He was spiritual not sensuous. Although like Carlyle he was subject to periods of depression, he was, in the main, full of hope and of faith in an invisible power under which all things work together for good. This sentiment he has well expressed in the lines which have been called Wordsworth's Creed :

For me, consulting what I feel within,
 In times when most existence with herself
 Is satisfied, I cannot but believe,
 That, far as kindly nature hath free scope,
 And Reason's sway predominates; even so far,
 Country, society, and time itself,
 That saps the individual's bodily frame,
 And lays the generations low in dust,
 Do, by the Almighty Ruler's grace, partake
 Of one maternal spirit, bringing forth
 And cherishing with ever constant love
 That tires not nor betrays.

If we go to Wordsworth to learn sympathy we must, I think, turn to another poet for initiation into the cultus of Life-patience. In this noble worship, if I mistake not, Robert Browning is our Pontifex. "The Ring and the Book" is an eloquent apology for human motives, a vehement and very successful attempt to make us feel how

easy it is for men, under various phases of impulse, to entertain most different opinions as to the right or wrong of the same sentiments and actions. A perception of the possible good in all men, almost in all deeds, is to be found more or less distinctly in all his writings. When once we perceive this great truth the spirit of anger and denunciation dies, and we become able to see things calmly, to reason and be patient. Of the fallibility of all human judgments Browning never ceases to remind us. He has for us, however, yet nobler lessons than these. He teaches us the continuity of time and absolute permanence of all moral force. He insists that evil is merely a negation and foretells the triumph of the good. In his hands death loses its terrors, for we are made to see that it can but change the body,—or to use Carlyle's phrase the clothing,—and cannot possibly conquer the life that is within. The gain of earth we are told must of necessity be heaven's gain too—in other words, the gain of time must be also the gain of eternity. Those who wish to know what I mean must read his “Abt Vögler,” “Rabbi Ben Ezra,” and above all, the “Grammarian's Funeral.” In speaking of Carlyle's contempt for Darwinism, I tried to show you that really these two were saying the same thing, and I am now desirous to extend that assertion both to Wordsworth and to Browning. They both, looking at their subjects from the spiritual side and using poetical language, assert precisely the same conclusions as those of Darwin. What is the following but the doctrine of “survival of the fittest” applied to morals :

To trace love's faint beginnings in mankind,
 To know even hate is but a mask of love's,
 To see a good in evil, and a hope
 In ill success ; to sympathise, be proud
 Of their half reasons, faint aspirings, dim
 Struggles for truth, their poorest fallacies,

Their prejudice and fears and cares and doubts ;
Which all touch upon nobleness, despite
Their error, all tend upwardly though weak.

Or again,

The evil is null, is nought, is silence implying sound ;
What is good shall be good, with, for evil, so much good more ;

I could easily produce hundreds of other quotations to prove that Browning is in a most emphatic manner an apostle of hope and teacher of Patience in its highest sense. I should do myself an injustice if I were to allow you to believe that this is the only lesson which I have got from his noble poetry, but it is the chief one, and for it I owe him a lifelong obligation. The same kinds of consolation and sources of hope are also often alluded to with most delicate force by Mrs. Browning. It is, I think, in *Aurora Leigh* that she expresses her longing wish

That no truth henceforth seem indifferent,
No way to truth laborious, and no life,
Not even this life I live, intolerable.

At another place in the same poem she says, beautifully :

There are nettles everywhere,
But smooth green grasses are more common still.
The blue of heaven is larger than the cloud.

And again :

I would be bold and dare to look
Into the swarthiest face of things,
For God's sake who has made them.

I have named the four authors to whom, chiefly, I have been myself indebted. It may easily be the case that others have found the same lessons as clearly brought home to them by other writers. To those who wish to find the teaching of

them all combined in one man, I would commend the writings of Emerson, but they must be prepared for a considerable reduction in vigour. With still further dilution, but with much gain as to sweetness and grace, the same may be found in the prose and poetry of Longfellow.

Whilst of the four whom I have named, it is true that each one has his own particular message, so that I have been able to assign to each a special vocation; it is also true that they have much in common. One and all are eager to face the facts of man's existence, and the conditions which surround it, and to see and to feel the reality of all that concerns alike his physical and spiritual life.

Let good men feel the force of Nature,
And see things as they are,

is the prayer of them all. Each one in turn has for himself felt that force, faced those facts, and arrived at a sure basis of hope. All of them are zealous for man's effort, and confident of man's reward.

One general remark as to these our prophet-poets and I have done. They are of no service to those who do not believe them, it is only upon those who will permit their ears to hear that their blessings can possibly rest. They did not coin their souls in order to gain from us mild approval or passing praise. They did not write merely to give us pleasure, or afford us material for criticism and debate. There is, I admit, a vocation still higher than that of prophesy, less didactic and more purely sun-like, which can lift us without our effort, and inspire us without our toil. In so far, however, as those whom I have named are prophets as well as poets, they claim from us the most earnest attention to the truths which they so earnestly propound. We must, on our part, make some attempt to translate those truths into actions if we would feel their force and know

their worth. We must seek their real meaning with painstaking industry, and when found, we must yield it such obedience as its due.

Now, Gentlemen, students of our College, in conclusion, I can but hope that none of you will consider that I have unduly neglected an opportunity of speaking to you on subjects directly pertaining to your education. I hope to have other opportunities for doing that. It has seemed best this evening to address you on topics which concern us all as men. I might have devoted the hour to eulogy of the Profession which you have chosen, but surely the Art of Healing needs no praise from me. So, also, I might have lauded the school which you have selected and paid compliments which would have been very sincere however blunt, to those at present concerned in its management. But here, again, facts speak for themselves, and you are familiar with them. I have preferred to try to find your motives for work and to give you strength and confidence in study. If now I were to sum up in one sentence what I have been suggesting it would be this, The secret of all noble life lies in belief, and the characteristic of all noble minds is the vigour with which they believe that which is true. Try to attain belief in the reality of all things, so shall you never want for motives, so shall you be able to live and work without hurry and without sloth. Finally, permit me to commend to you this formula—prize strength, love beauty, practice self-denial, and be patient.

INTRODUCTORY ADDRESS

AT THE

OPENING MEETING OF THE SESSION 1883-4,

October 11th, 1883.

By JONATHAN HUTCHINSON, F.R.S., President.

GENTLEMEN,—We begin to-day the fourth session of the Ophthalmological Society of Great Britain. In the first place I must thank you heartily for the honour you have done me in electing me your second President. Appreciating this honour most highly, I yet accept it with much misgiving, more especially when I remember who has been my predecessor.

In Mr. Bowman you have enjoyed the services of a President of unequalled fitness and ability—of one, indeed, who had already conferred inestimable benefits on ophthalmic science. His acceptance of the office of President at once secured the success of our infant Society, and in his recent resignation of it we have sustained a heavy loss—one which, I am well assured, it will be quite out of my power, in any degree, to make you forget. The chief satisfaction which I have, in venturing to accept your invitation to succeed him, is the knowledge that he still takes the warmest interest in our affairs, and that I shall always have the advantage of his advice and help.

It will be my duty to mention to you directly, facts which will prove that, although Mr. Bowman has resigned the nominal presidency, he still occupies in relation to us an almost paternal position. Long may he live to do so!

We meet this evening, as you will have observed, in

rooms which have been made much more commodious since our last session, and in connection with this subject I have to give you some important items of information. The first is, that the Medical Society of London, whose tenants we are, on entering upon their greatly improved premises, felt themselves obliged very materially to increase our rent. There was nothing in the least unreasonable in this ; indeed, our landlords have throughout acted towards us in a liberal spirit. The proposed increase was, however, in respect to our finances, a very heavy one, and as the Council was desirous to collect a library, and to form a museum of instruments and appliances—both objects demanding outlay,—we found ourselves for a time in a position of great difficulty. It is almost certain that the Society could not have afforded to continue in these rooms and develop itself in the proposed directions had it not been helped by an act of scientific beneficence not often surpassed.

Having acquainted himself with the facts, and noting our position, our ex-President made an offer to the Council to himself undertake the cost of purchase of all fittings necessary for the museum and library, and further, to make a gift to the Society annually, for twenty years, of the sum of £50, in order to defray the expenses of rent of rooms. Need I say that the Council on your behalf thankfully accepted Mr. Bowman's munificent offer, and I have now the pleasure of informing you that we are, in all probability, rent-free for twenty years, and shall be able to devote the whole of our income from subscriptions to the publication of our annual volume. I am sure that you will receive this most liberal gift of your past President as one in the highest degree worthy not only of praise in the present, but of imitation in the future. The pecuniary advantages which it confers upon us are solid, extensive, and durable ; but, warmly as we appreciate them, I believe I may say that those who have been most closely associated with the early years of our Society derive yet greater pleasure from the fact, that one so

competent to judge, should in so emphatic a manner have crowned their efforts with his approbation.

I have next to allude to a remarkable coincidence. Mr. Bowman's offer was made only about a month ago, and quite unexpectedly to us all, whereas for at least three months before this, and wholly unknown to him, the Council had had under consideration a proposal to recognise his pre-eminent position in respect to ophthalmology in Britain, and the invaluable services which he had already rendered to our Society. It was from Dr. Gowers that the suggestion had first come that we should found a lectureship to be known as the Bowman Lecture, but it was no sooner mentioned than it was received with unanimous approbation.

I am anxious, for reasons that will be self-evident, to make it clear that the Council's endeavour in this slight manner to do honour to Mr. Bowman, and his liberal endowment of the Society, had no connection one with the other, although the two projects ripened about the same time. Dr. Gowers' proposal has been several times discussed in our Council meetings, and should it meet with your approbation, as I feel sure that it will, the lecture in question will be founded forthwith. Without wishing unduly to bind the executive in future years, the present proposal is that a Bowman Lecturer shall be appointed each year, and invited to prepare for us a critical summary of the best extant information upon some special subject to be selected by the Council, or, if not selected, approved by it.

This lecture will probably be an annual one, and will be delivered at a meeting specially appointed for that purpose. We hope in it not only to permanently associate with our Society the name of a great man, but also to contribute each year something real towards that "advancement of knowledge for the good of man's estate," which has been Mr. Bowman's lifelong aim.

With this statement I end, gentlemen, the announcements which it has been my most pleasing duty to make

to you, but I purpose before sitting down to trespass upon your attention with a few further remarks on our general position and the possible scope of our future work.

I think that we may now fairly congratulate ourselves that the organisation of our Society, if we regard it simply as providing means for the furtherance of research in ophthalmology, is well-nigh perfect. We shall have regular meetings in commodious and central rooms, at which any subject which is brought forward will be certain to receive the attentive criticism of an audience, than which none exists better qualified for the task. We invite in the freest possible manner the production of all new facts, opinions, and suggestions, whether relating to extended series of observations or isolated cases. All that concerns the eye, whether in health or disease, concerns us, and we shall be thankful alike for the single case and the elaborate paper. Nor is there, I am happy to say, any spirit of exclusiveness as regards membership with us. We shall willingly accept the help of all who take an interest in our pursuits. Those who had the largest share in the formation of this Society were careful that it should have a wide basis, and, thanks to their foresight, it has now the good fortune to include amongst its members many, physicians, surgeons, and others engaged in general practice, who are not, and never have been, in any sense, specialists.

To say nothing of the original contributions which we have had from some of these, their help in our debates and their services on our committees have been, and will be in the future, simply invaluable. It is true that we have not yet a library of reference, or a museum. But the first of these desiderata will, I doubt not, soon be supplied, and the other will be put in course of formation to such extent as may suitably come within our lines of work. We shall probably never attempt the formation of a pathological collection, since we have no convenience for its preparation or its display, and there exists, besides, at other institutions, ample provision in this direction.

We do, however, contemplate the formation of a collection of instruments and appliances, and to this object Mr. Bowman's endowment will, as I have said, be in part devoted. Probably also we shall make gradually a collection of drawings and other forms of graphic illustration. These can be easily classified and stored for reference in the drawers of our library. Should it occur to any of our more wealthy friends to emulate Mr. Bowman's noble example, I cannot, for my own part, think of any object to which a second endowment could be more usefully devoted than to the formation of such a collection.

Morbid conditions of the eye, whether external or revealed by the ophthalmoscope, lend themselves with peculiar facility to the artist's skill. If we had the funds I would suggest that, under the auspices of a committee, we should copy, collect, and classify, from all available sources, private and public, published or otherwise, all such illustrations of eye disease as are passably good in execution, and duly authenticated and described. With but few exceptions I would leave aside all in which the history of the individual case is omitted. If this scheme were completed we should find, if I am not mistaken, that we were in possession of a sort of clinical museum which would prove of very great use alike to students and to all engaged in original research. I certainly count this object as chief among the desiderata for which adequate provision has not yet been made.

Hitherto I have been speaking of our arrangements and organisation as a Society for the improvement of knowledge in our special branch. To those who, with me, believe that it would be difficult, in any material degree, to alter these arrangements for the better, it is, I may repeat, a source of great satisfaction to know that they have received the emphatic *imprimatur* of our first President, than whom there is no man living so well qualified to judge.

The improvement of ophthalmic knowledge is unquestionably our first, and by far our principal duty. I

cannot but think, however, that it is possible that in the future such societies as ours may find another kind of work open to them, which is only second in importance.

I allude to the systematic and strenuous endeavour to diffuse rapidly amongst the profession at large, for the prompt benefit of our patients universally, all items of new knowledge which may have been obtained.

There are many directions in which thoughtful help might be given towards this end. We may, in the first place, endeavour to induce as many as possible to join us, and attend our demonstrations and discussions and receive our volumes. We shall not, however, in this way reach many excepting London residents.

It is perhaps possible that something might be done to make some of our meetings, and the reports of them which appear in the journals, more valuable to the bulk of the profession, by becoming less definitely special than they now are. We might, for instance, bring forward for discussion, occasionally, the commoner forms of eye disease—such as are scarcely likely to be often made the themes of original communications. Not only would this help others, but it is very desirable for our own good that we should occasionally make recapitulation in public of our knowledge of common things, and thus ascertain how far our opinions have advanced towards unanimity.

There is another branch of the same topic on which I incline, if you will permit me, to enter into a little more detail, since it offers possibly a sphere for much useful work in the future on the part of societies like ours. It is one, indeed, to which perhaps this Society in particular is more specially called than any other. I refer to the promotion of what may be named *every-day therapeutics*. It is obviously quite possible that the knowledge of diseases of the eye might be cultivated by a few up to a point of very high excellence, and with great finish of detail, and yet remain a possession of the specialist, and benefit but little the family practitioner, and the public his patients. In some degree this state of things is un-

avoidable, and in some departments of our practice we cannot hope to ever escape it. Still, however, it will be admitted by all to be a matter of regret. So far as we can do it, it is our duty to make such knowledge popular—to diffuse it over an area the widest that we can obtain. A practical knowledge of astigmatism is not to be expected from a general practitioner; possibly not even from all who are engaged in the treatment of eye diseases as a specialty. The attempt to use the ophthalmoscope for purposes of diagnosis, although quite possible to a large section of the younger part of the profession, enjoying constant opportunities and fresh from hospital training, would probably, to by far the greater part, prove to be a source of error rather than a help.

Skill in the diagnosis and, as a necessary consequence, in the treatment of a not inconsiderable group of rare diseases of the eye, must always, despite any development of education which it is reasonable to hope for, and any artificial aid which can possibly be given, remain the possession of the specialist only. But it is otherwise in respect to a majority. Almost all the examples of the commoner forms of eye disease come under the care, in the first instance and often throughout, of those who are not specialists, and have perhaps never even had any training in an ophthalmic hospital. Circumstances over which no one has any control render this inevitable. Whether or not the surgeons concerned desire it, they must perforce take charge of "eye cases" as well as of others. It is in reference to practitioners so placed that I would suggest that our Society has possibly a duty to perform. If I trouble you with a few examples, I shall probably be best able to convey my meaning.

Concerning the treatment of syphilitic iritis, there is probably but little hesitation or difference of opinion amongst specialists, and perhaps I could hardly mention another disease respecting which the opinions of specialists are more widely known and accepted. That atropine should be used from the first, frequently, freely, and in

strong solution, and that mercury and iodide of potassium are very useful and ought always to be given, but in no degree compare in importance with mydriatics, I take to be the acknowledged canon. It would be easy to prepare an explicit *schema* for the treatment of this disease, giving the exact strength of the atropine, the frequency of its application, the precise dose of the mercurial, and suggesting a few of the more important means which help success, such as a purgative, leeches to the temples, and low diet. This might be done in ten lines, and so printed in a visiting-list or pocket-book that it should be readily accessible to all. It would be better that such a schema should be propounded under the auspices of a Society than that it should come from an individual. In many parallel instances, the discussion and examination which such *schemata* of treatment would receive at the hands of our Society would, no doubt, be of great use in perfecting them, as well as in adding to their authority.

I do not doubt that there are, at the present moment, whilst I am speaking to you, in the homes, the schools, the workhouses, and the hospitals of England, some thousands of children who are suffering from ulcerations on the cornea, attended with intolerance of light, causing the patient great distress and annoyance through many months, and destined often to leave disfiguring and incapacitating scars. If my own experience may be trusted, I believe that three fourths of these would be almost well in the course of a fortnight under the use of a very weak yellow oxide ointment. Many of them, no doubt, are getting it, but a considerable majority probably are not; for this plan of treatment is not yet universally acknowledged among specialists, and certainly not very widely known in the profession.

If this Society could, after an examination of the subject, determine upon the recommendation of an explicit formula which would be likely to result in the prompt cure of these very troublesome cases, it would confer an immense boon upon the public. Such a formula, so

recommended, would be copied into every medical journal and into every manual. It would be reprinted over and over again, and would become the property of the whole profession.

Is it not somewhat humiliating to reflect that if a quack were to bring out a very weak Pagenstecher's ointment, give it a telling name, and push it into notice as a specific for chronic inflammations of the eye, he would be a public benefactor? No doubt it would often be used in error, but it would even then do little or no harm, and I have not the least doubt that the balance of gain would enormously preponderate. My own experience has been, that since I knew the virtues of this ointment I have been able to abandon almost entirely the use of blisters, setons, and like painful measures, and to effect the cure in a tenth of the time. I have reason to think that a large majority of ophthalmic specialists have had a like experience. Yet we hesitate to come boldly before the general profession and announce loudly an important item of progress. We fear to boast, we dread to impair the scientific spirit by the formation prematurely of general rules; and, seeking to quiet our consciences by reminding ourselves that after all the thing is no secret, we do nothing further in the matter. Our reticence is a loss to the nation, it is an injury to hundreds and to thousands whom the benefits of modern ophthalmological science might reach if we would only consent to throw away our fastidiousness. Is it not a frequent failing among the more scientific part of our profession to become superfine? We dread the spirit of the charlatan and the self-seeker so much, that we come, like David when in presence of the sinner, to "hold our peace *even from good.*" In the individual, scrupulous care in these respects is most meritorious; nothing is less to be desired than that those who believe themselves to have made therapeutic discoveries should deem it their duty to proclaim them ostentatiously. Let them be brought forward in the first instance quietly, and under the cognisance only of those skilled to judge of them.

But the fact that it is meritorious in individuals to abstain from pushing their favorite remedies, only throws the duty, to which I have been alluding, the more definitely upon public bodies like ourselves. No one could impugn our motives or doubt our sincerity, and our verdicts would be received not certainly as final, but as entitled, at any rate, to a temporary acceptance.

Let no one suspect me of wishing to stereotype knowledge or to damp the ardour of any skilled person in the endeavour yet further to improve our therapeutic resources. There is no fear in that direction; and what I am concerned to assert is this, that nine out of ten of the practising part of the profession would most thankfully receive from this Society detailed *schemata* for the treatment of various typical forms of eye disease. Let me further add—without, I hope, hurting anyone's feeling—that I am sure that the use of them would tend immensely to the benefit of their patients as compared with the extemporised prescriptions now employed. It is not in the power even of the most laborious of those engaged in family practice, to keep their minds well stored with details respecting the management of diseases which, although very common with us, are rarities to them.

I might easily mention a number of special types and forms of eye disease—purulent ophthalmia, rheumatic iritis, episcleritis, catarrhal ophthalmia, glaucoma, and the like—for which definite schemes of treatment could easily be laid down. It will, I have no doubt, be objected, that, after all, successful treatment depends upon the correctness of the diagnosis. This statement is almost as obvious as was the famous injunction to “first catch your hare.” It is no reason that because diagnosis is difficult, therapeutics should be left in a muddle also.

I might urge further that I believe, working on the same lines, this Society might do much to put the diagnosis of eye diseases more easily within the reach of British practitioners in general.

There is no one present who has not been pained over

and over again by having to treat cases of glaucoma which were brought to him too late. In spite of all that has been done by specialists, and in spite of the fame which iridectomy cures have obtained, it is still the fact that a large proportion of cases of acute glaucoma are unrecognised during the first fortnight by those under whose observation the patients come. Practitioners of the most scrupulous care, of wide general information, and the most conscientious regard for their patient's good, are yet very commonly misled by the acute congestion and severe constitutional symptoms which often attend the early stages of this disease.

It was my fortune, some years ago, to operate upon three cases of this kind in one week, in all of which the proper time for interference had been allowed to pass by, on account of the patients' severe general illness.

In one instance I became acquainted with the facts of a case in which a benevolent country surgeon, aided by two or three friends, was himself maintaining a lady who had lost her sight, and consequently her occupation, from double acute glaucoma. He had himself attended her from the beginning, and when I gently hinted at the possibility—to me, a practical certainty—that iridectomy at the proper time would have saved the lady's sight for the rest of her life, he promptly replied “that the eyes were so much inflamed in the first instance, and the patient so ill, that he was quite sure I should never have thought of operating.” I said no more, for it would have been cruel to tell him that these were the very symptoms which denoted the necessity for an operation.

Some years ago, in the early days of the keratome, I felt so strongly on this subject that I had some thoughts of engaging a full page in the ‘Lancet’ for a big red-lettered anonymous advertisement, so staring that all must read it, stating in a dozen words the symptoms and inevitable result of glaucoma, together with the certainty of its cure by operation.

And now, looking back upon such impulses of enthu-

siasm, I do deliberately declare my conviction that a Society like our own would have been more than justified in taking such a step. At that time acute glaucoma probably had, on British soil alone, its daily victim, whom it left in irrevocable blindness. In the present day the number has been greatly diminished, but it is still, no doubt, very considerable. Our confidence in the remedy which we then hailed has remained unshaken; and it is most certainly a very melancholy thought, that there are thousands now living without sight who might have saved it very easily had there existed any efficient means for the rapid diffusion of the new knowledge.

I must not trespass further upon your patience in this matter. Briefly, what I desire to urge is this, that we ought not to be content with doing our utmost to make knowledge perfect, and to secure its application in our own immediate spheres of action, but that it is well worth a thought whether Societies like our own have not duties to perform in respect to its diffusion. I will not for a moment doubt that a subject so important will receive from you such attention as your judgments may deem it entitled to.

Is it too much to hope that something of the nature of a compendium of ophthalmic therapeutics may sometime be prepared, which shall bear the authority of a Society's consensus? Such a code should of course be destined to modification from time to time, but it would probably from the first be a great advance upon the statements of any individual, both in explicitness, in brevity, and in the amount of practical experience which it would summarise.

Should the Society see its way in the future to any action in this matter, much collateral advantage might be expected by the more detailed attention to therapeutics which would be given by the committees appointed to report.

Had time permitted, I might have ventured to bring before you a few other suggestions as to work which the

Society might undertake collectively—such, for instance, as a systematic examination of symptoms with the object of defining and describing them more accurately ; of preparing detailed lists of the more rare types and forms of disease, and giving to each its concise description ; and possibly, after this were done, of preparing nosological lists which might assist the labours of hospital registrars.

I have occupied, however, already much more time than I had intended, and must not detain you any longer from the proper work of our meeting.

ON

OPHTHALMOPLÉGIA EXTERNA

OR

SYMMETRICAL IMMOBILITY (PARTIAL) OF THE EYES, WITH PTOSIS.

BY

JONATHAN HUTCHINSON, F.R.C.S.,

SENIOR SURGEON TO THE LONDON HOSPITAL; CONSULTING SURGEON TO THE
ROYAL LONDON OPHTHALMIC HOSPITAL; AND SURGEON TO THE
BLACKFRIARS HOSPITAL FOR SKIN DISEASES.

Read February 11th, 1879.

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1879.

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Received December 10th, 1878—Read February 11th, 1879.

THE cases to which I have to ask the attention of the Society are characterised by a very peculiar group of symptoms. Drooping of the eyelids, so as to give to the face a half-asleep expression, is usually the first, and it is soon accompanied by weakness of all the muscles attached to the eyeball, so that the movements of the latter become much restricted, or even wholly lost. The condition is usually bilateral, though it is not always exactly the same in degree on the two sides. Its symmetry probably denotes that it is of central origin. It by no means always happens that all the ocular muscles are alike affected, or that they are attacked simultancously, still it is a very marked feature of the malady, that the muscles fail in groups, and not singly. Non-symmetrical paralysis of single ocular muscles is, of

course, very common, especially in connection with syphilis and locomotor ataxy, but such cases are to be distinguished from those which I am now describing: first, by the fact of non-symmetry: secondly, by the early completeness of the paralysis; and, thirdly, by the ease with which, very frequently, they are cured. In a majority of them, there is, perhaps, good reason to suspect that a gumma in the nerve trunk is the cause. In the symmetrical cases now under consideration, however, the changes probably begin centrally; they are usually slow in progress, and are often difficult of relief. They agree with the single nerve cases in that they occur chiefly to those who have had syphilis. Although I have ventured to speak of immobility of the eyeballs, I by no means wish to imply that it is usually complete. On the contrary, incompleteness in the degree of paralysis is almost as marked a feature as is the tendency to affect many muscles at the same time. Although the eyelids droop, there is seldom complete ptosis; great limitation of the range of motion of the eyeballs is more common than fixation. The degree, however, varies with the stage, and at a late period the paralysis may be absolute. The third, fourth and sixth nerves are of course those which are involved, but not unfrequently in the early stage one or more of these may wholly escape. Occasionally the optic nerve itself is involved, and sight is lost. I am making these statements from the observation of only a limited number of cases, for the condition is but seldom seen. I have as yet witnessed only a single fatal case, and in it evidence was afforded of extension of disease downward into the motor tracts of the cord, and the man lost the use, first of his upper, and subsequently of his lower extremities.

In a former paper¹ I have ventured to propose the name *Ophthalmoplegia Interna*, for cases supposed to depend upon disease in the lenticular ganglion, in which the internal muscles of the eye (the iris and ciliary muscles) are together involved in paralysis. If in contradistinction with these, I may now be permitted to suggest that of *Ophthalmoplegia*

¹ 'Med.-Chir. Trans.,' vol. lxi, p. 215;

Externa for those in which the external muscles of the eyeball fail in power, it must be with the full admission that these latter often, indeed usually, include the internal muscles also. With paralysis of the third nerve, the circular fibres of the iris and the ciliary muscle must of course fail, but the defect in these parts in this class of cases is, usually, as I have asserted of the recti and levators, incomplete. In this incompleteness, at any rate in the early stages, it differs from what we see in cases of supposed disorganisation of the lenticular ganglion. Nor does it always happen, even when in ophthalmoplegia externa the third nerve root to the ciliary ganglion is involved, that the vaso motor filaments suffer too, and in this we find a further difference from what would be expected if the ganglion itself were attacked.

The cases in question are probably closely allied in nature to what is known as progressive muscular atrophy, their peculiar feature being, that only one special set of muscles (or rather nerves) is at first attacked. We have, probably, in them, a very close parallel to the so-called bulbar paralysis, the labio-glosso-laryngeal paralysis of Duchenne. In it, as in ophthalmoplegia externa, central degeneration-changes occur, and the result is the paralysis of a set of associated muscles. It may be plausibly conjectured, that the initial lesion is inflammation of the nuclei of the affected nerves, which, in a slowly serpiginous manner, creeps from place to place, along certain definite anatomical paths. Within certain limits its directions of spreading, and its progressive tendency may vary in different cases, but speaking generally, the cases are remarkably same in their features. In exceptional instances, definite symptoms of locomotor ataxy are present, and in others still more rare, the fifth nerves, or the seventh, or even the eighth may be involved. Although, so far as I am aware, this group of cases has not as yet found any record in English medical literature, it has not escaped notice in Germany. Graefe described examples of it in 1867, and employed the word ophthalmoplegia in reference to them, and Eulenberg in his work on 'Functional Nerve Disorders' devotes a short chapter to the subject, which, however, con-

sists chiefly of an abstract of Graefe's communication. By both these authors it is spoken of as sometimes syphilitic, and sometimes rheumatic, and occasionally without assignable cause.

The clinical part of my paper consists of the narrative of fifteen examples of the malady. Several of these cases extend over many years, and are very detailed. I have done my best at abbreviation, but am very conscious that I shall still need the Society's indulgence in this matter.

CASE 1.—My first case occurred in 1862, and has special interest, because I am able to produce a photograph of the patient. A printing-house porter, aged 52, applied at Moorfields almost blind with white atrophy of the optic nerves. His eyelids drooped a little on both sides, and he was obliged to elevate his head, and use the occipito-frontales in order to see. The eyes were almost fixed in the orbits. The photographer to whom I sent him, reported him "an unusually good sitter, for he never moved his eyes." Both eyes diverged, the left especially. On both sides all the third nerve muscles were paralysed completely, excepting the levator. On the left side, the fourth nerve was paralysed, and the only muscle which he retained was the external rectus. On the right side, the sixth nerve was paralysed, and the only muscle which he retained was the superior oblique. In both eyes the discs were white and the retinal arteries small. He was not deaf, his faculties, excepting sight and smell, were good, and he had excellent health. He had never had "a fit" nor any limb paralysis, and he had never suffered from pains in his head.

The case was remarkable on account of the long interval between the paralysis of the muscles, and the failure of sight. The progress of symptoms in the early stages appeared to have been very slow. The history as to syphilis was that in 1834 (that is thirteen years before his first eye-symptoms) he had a long treatment by mercury for a chancre with buboes. He was salivated. No secondary symptoms were remembered. In addition to the eye-symptoms his smell had failed,

Ammonia made him sneeze, but he could detect no odour in hyoscyamus. He considered his taste perfect.

This man remained under my observation from January, 1862, to July, 1864. In August, 1863, he began to complain that fluids in swallowing came through his nose. His speech was thick, and the palate appeared very deficient in sensation. His pupils were quite motionless, and of medium size. His uvula could not be made to move. In July of 1864 he was in the same state. The left external rectus, and the right superior oblique, still acted well. The muscles of his palate did not act, but tickling it, although but little felt, caused sickness. His cheeks hung loose as if partially paralysed, but nothing positive could be proved. He could still whistle.

After 1864 this patient ceased to present himself. From inquiries at the workhouse I believe that he died in 1866.

CASE 2.—My second case is that of a young physician who, in 1871, put himself under my care for symmetrical paralysis of the third nerves. He was thirty years of age, and had been treated seven years before for a chancre (by the late Mr. Gascoyen). No secondary symptoms followed, but he had since had frequent reminders in the form of psoriasis palmaris, &c. He had also been liable to “rheumatic pains,” which were always relieved by iodide of potassium. About a week before I saw him he began to be troubled with morning sickness, then his eyelids felt heavy, and next they drooped, and he began to see double. When he came to me he had slight ptosis on right and complete on the left. On the right the pupil was dilated and fixed, and accommodation lost; whilst in the left the pupil was normal and accommodation was perfect. The internal rectus was very defective on both sides. On the left the superior and inferior recti were almost wholly paralysed, whilst the superior oblique was perfect. On the right these recti were weak, but only to a slight degree, but a fortnight later the ptosis on this side also became complete, and all the recti, excepting the external, were absolutely paralysed. Both eyes diverged, and, on the right side, he had entire loss of accommodation.

At this period he complained much of "horrible museular pains" in his legs, which were relieved by exercise. He had also morning sickness, almost exactly like that of pregnancy, and some frontal headache. Subsequently there was general dulness of sensation in the lower extremities. Dr. B. remained under drug treatment almost continuously for two years and a half. We gave mereury in various forms, and the iodide in very diverse doses. Sometimes, after an interval, five-grain doses produced very definite results, and at one period he had got up to *an ounce and a half in the twenty-four hours* without feeling any discomfort beyond lassitude. The sum of our experience was, that the iodide did more good than mercury, but that it was useless unless the dose was frequently and liberally increased. The functions of accommodation and the use of the internal recti varied much from time to time, and always afforded us a good test of the efficiency of treatment. I never, in any other case, pushed the iodide to anything like the extent to which it was given in this instance. The result, however, was most satisfactory. Dr. B. lost all his ataxie symptoms, he regained perfect accommodation, and most of the ocular muscles recovered their power. He is now, seven years after the conclusion of treatment, in good health, and has never been threatened with any relapse. It should be added that we had the advantage of sea-air at the time that the largest doses were given, and without its aid I much doubt whether the result would have been as satisfactory as it was.

CASE 3.—The following case was under my observation from its commencement, in 1869, to its end, in 1876, and it is of especial value as the only one in which I have obtained a *post-mortem*. The symptoms, which began with ocular paralysis and amaurosis, were slowly progressive, and the case in part resembled locomotor ataxy, and, in part, progressive museular atrophy.

Robert S—, æt. 48, a very healthy-looking man from the country, formerly a policeman, now a gardener, was sent to me by my late colleague, Mr. Dixon, in March, 1869. He

had a slight defect of his right sixth nerve, and could not abduct the globe well. There was slight convergence, and he complained of seeing double. The right pupil was rather larger than the other. He complained that he did not see well by artificial light, and that he could not read long at a time. He had enjoyed good sight until six weeks before, and had never used glasses. I found that without glasses he could, by effort, with left eye read No. 10, but with right only 14. For a minute or two he could puzzle out smaller print, and it appeared that his defect was wholly due to weak muscles of accommodation, for with +16 glass he read No. 1 easily with either eye, and his distant vision was almost perfect.

I could not arrive at any opinion as to the cause of the rather sudden onset of symptoms. He denied having ever had syphilis, and he had been married early and had seven living children. He had never had any injury to his head more serious than the blows to which policemen are liable, nor had he had gout. Of late he had suffered a good deal from what he called "rheumatism" between his shoulders and had also had giddiness and much pain across his forehead. I prescribed iodide of potassium, and during a six weeks' treatment with it he made some improvement, and I then lost sight of him. I am sorry that I cannot state the exact degree or kind of improvement, but he became able to continue his work as gardener with comfort, and, as he lived at a distance, he was anxious to avoid journeys to town.

Four years later, in October, 1873, Robert S— again came to me at Moorfields. In the interval his sight had steadily deteriorated, and with the right eye he had now only bare perception of light, whilst with the left he could scarcely spell No. 20. He could not abduct either eye, and the right was habitually crossed inwards. There was no positive paralysis of any of the other ocular muscles, but they all acted feebly, and he had the same sleepy look observed in other cases, from not being able to keep his upper eyelids properly elevated. He could, however, by effort, lift his eyelids. His left internal rectus seemed weaker than the rest. The optic

dises were both very pale, and the artery and vein much reduced in size. The atrophy was more advanced in the right. He still enjoyed good health, and, in spite of being almost blind, still acted as gardener and groom. I could not make out much as regards other evidences of disease of the nervous system. He was liable to attacks of severe pain in his forehead, sometimes lasting a week or more. For seven or eight years he had been liable, he said, to cramp in his legs at night. His bowels were constipated, but not extremely so. At this date his pupils did not act in the least—the left being of medium size, and the right rather larger. He had no habitual headache. I now prescribed mereury and pushed it to ptyalism, but with no definite benefit.

During the spring of 1874 I saw him repeatedly in the country, and about March the following note was made:—Pupils motionless. External recti paralysed, and all the others imperfect. The superior oblique appear to be also weak, but as he can still use the inferior recti a good deal it is difficult to test them. His bowels have been very costive, and he complains much of tightness round the abdomen as if a strap were round him, and also of numbness in the skin of abdomen and face. There is also slight numbness in hands and a little in feet. Up to this date the man had been able to get about, and used to drive himself in a pony carriage a distance of seven or eight miles to see me. Not long afterwards, however, an aggravation of symptoms took place and he became confined to his house. After this I did not see him again. I was informed that he had a “sort of choking fit” and soon after became absolutely blind. His extremities next failed him, and he had to keep his bed. He now suffered from dreadful pain in his head, and was frequently “out of his mind,” and liable to use most violent language. He could speak well and swallow easily, and used to eat largely. His lower extremities became quite useless and were usually “icy cold.” In this condition he died in May, 1876, about seven years after the commencement of his symptoms.*

* The following particulars were kindly obtained for me by Dr. Sloman, of Farnham. “About Christmas, 1874, loss of power in his arms was

Two very remarkable coincidences occurred in this case and by their aid I am enabled to complete its narrative. They are so peculiar that I must ask the Society's excuse if I briefly mention them. I had been very desirous to know whether this man had suffered from syphilis; as stated, he stoutly denied it, and he appeared to be straightforward. I got him to bring me two of his children and neither of them showed anything in the least suspicious. So matters stood when in April, 1876, Dr. Horace Jeaffreson of Wandsworth wrote to me that he had under his care a maid-servant who was the daughter of a man in whom I had taken much interest and that the girl was now suffering from inflamed eyes. He sent to me Robert S—'s eldest child, a girl of twenty, with *notched teeth and a most characteristic condition of syphilitic keratitis*.

This girl, who became my patient at Moorfields, informed me from time to time of her father's condition. I was very anxious to get Dr. Hughlings Jackson to see him, and for a year or more we had it in contemplation to go some day to Farnham and ascertain the present state of his symptoms. One Saturday afternoon in the spring of 1876 we accomplished our intention, and having found the man's cottage ascertained that he had died the day before. After much persuasion we succeeded in getting permission for a post mortem, and brought his brain home with us.

Dr. Gowers was kind enough to make for me a detailed microscopic examination of the cerebral nerves and their nuclei, and I append his report. It will be seen that degenerative changes precisely similar to those of progressive

noticed; he would drop a cup or anything he was trying to drink from. In January, 1875, his legs became weak, and he took to bed at the end of January or beginning of February; he was constantly in bed after that. During this time his arms and legs would from time to time "fly up" (as his wife expresses it), and as suddenly drop again. His lower limbs were constantly cold. He had no constant paralysis of the sphincters, but very occasionally urine was passed unconsciously. The bowels never acted at all without medicine. He could swallow perfectly, and also speak and taste. He had no paralysis or loss of sensation in muscles of face. He used to suffer much from headache."

muscular atrophy were found. These changes implicated the optic nerves and the third, fourth, and sixth. The fifth were slightly affected, but all the others were healthy. We had not been able to obtain any part of the spinal cord. From the nuclei of all the nerves mentioned the cells had disappeared.

Examination of the brain by W. R. Gowers, M.D.

The brain, when received, was somewhat softened from commencing decomposition, and the following facts are all that could be ascertained with certainty. The cranial nerves were examined microscopically, in the fresh state; the pons and medulla after hardening.

Nothing abnormal was observed in the convolutions. The corpora striata were apparently normal, the only exception being the presence of a hyperæmic patch in the left lenticular nucleus. The optic thalami were of normal size and consistence, except that the posterior tubercles were perhaps a little smaller than normal, and a little softer. Microscopical examination did not show any special change. The corpora quadrigemina were of nearly normal size; the posterior only being distinctly smaller than natural, but on microscopical examination they appeared healthy. The pons and medulla were of the usual size, and the only abnormality was an unusually deep central sulcus of the floor of the fourth ventricle, but this was found, on further examination, to be independent of any change in the structure of the pons.

Cranial nerves and their nuclei.—The *olfactory nerve* appeared normal. The *optic nerves*, and chiasma were uniformly grey, but of fair consistence. The optic bracts were also grey, but in places presented a white almost nacreous striation. Microscopical examination of the nerves and tracts showed many fat globules and degenerating fibres, but also a large number of healthy fibres. *Third nerves.*—Smaller than natural, grey and translucent. Very few healthy fibres could be seen in them; some fibres were seen undergoing degeneration, and there was a large number of

connective tissue nuclei. In the crura cerebri the passage of the tracts of fibres of origin was indicated by lines of connective tissue fibres in which scarcely any nerve fibres could be seen. Their nuclei beneath the nates presented striking changes. Almost all the large multipolar nerve cells had disappeared, two or three only were to be seen in each section. A few cells were seen of some size, but without processes. Others appeared represented by minute angular cells not larger than connective tissue nuclei. The latter were very abundant throughout the tract.

Fourth nerves.—No trace of these could be seen. They had probably become reduced to fine connective tissue threads, indistinguishable from the fibres of the pia mater. Their nuclei beneath the testis presented a similar degeneration to those of the third nerves in the anterior part of the same tract.

Fifth nerves.—The upper fibres of the large root appeared healthy, but the lower fibres had a grey appearance and presented granular degeneration and segmentation on microscopical examination. Within the pons there was little recognisable alteration in the fibres of the nerve, and the nucleus was for the most part normal. Here and there were patches of disintegration. The nucleus of the motor root of the fifth was in all respects normal.

Sixth nerves.—These were reduced to fine grey threads in which, under the microscope, scarcely a single nerve-fibre could be seen. They were made up of nucleated connective tissue, with here and there a row of granules to indicate the position which had been occupied by a nerve fibre. Within the pons lines of connective tissue alone indicated the course of their tracts of origin to the nuclei beneath the eminentiæ. These, the so-called "conjoined nuclei" presented general degeneration; most of the large nerve cells had disappeared and only granules, nuclei, and small angular cells remained.

Facial nerves.—Perfectly normal, both in their trunks and roots of origin within the pons.

Auditory nerves and nuclei normal.

Glosso-pharyngeal nerves and nuclei normal.

Pneumogastric nucleus normal.

Hypoglossal nucleus normal.

Throughout the medulla, pons, and corpora quadrigemina the perivascular erosions, so frequently met with, were very large and numerous. In the lower part of the floor of the fourth ventricle there were some areas of disintegration, in the grey substance just beneath the lining membrane; and the surface, partly from this cause, was more than usually irregular. One such area appeared to have been caused by a small hæmorrhage.

Remarks.—The degeneration of the nerve roots, and disappearance of the nerve cells from their nuclei of origin, are precisely similar to the changes seen in other parts in progressive muscular atrophy. There was no indication of pressure upon the nerves, nor of any acute change in their nuclei, nor in any centre. The disintegration and connective tissue changes are constantly met with in the grey matter of the cord in progressive muscular atrophy.

CASE 4.—In the following case no history of syphilis was acknowledged. It was, however, very closely similar to the others which were syphilitic; and when we remember what occurred in the preceding case in this matter, we cannot trust much to negative statements.

Abstract.—Almost complete paralysis (not equal) of all the ocular muscles, with the exception of the inferior rectus on right, and external and inferior on the left. Slow failure of sight in both eyes, but not symmetrical. Atrophy of discs with but little diminution of central vessels. A feeling of tightness across forehead and great irritability of temper. Patient a man of middle age in good health. Insanity threatened.

William M—, a florid healthy-looking man, æt. 45, came under my care in January, 1875. He considered that he ailed nothing whatever excepting his loss of sight. His pupils were of medium size and quite motionless. With his left eye he could see a hand, but could not count fingers, whilst with the right he could just spell out No. 20. His

manner was slow and dull; he answered me in monosyllables, and his wife said that at home he would often sit for hours and not speak. He slept much. He complained occasionally of dizziness, and said that he frequently had "a sort of tight pain" across his forehead. He had no pain in his limbs or joints. The account which he gave of his first failure of sight was, that it occurred when he felt quite well. He began to find his eyes dull, and they felt as if sand were in them. He had no sickness and could walk well. The left eye failed first. He attended at Guy's Hospital in 1873, and took phosphorus and nux vomica. A note on his Guy's paper, stated that "the optic discs were white and atrophic, blood supply to retina good." In June, 1874, he had an eruption on his legs, which he thought was due to the medicine. He describes it as purpuric.

When he came to me his expression was somewhat sleepy, from his not opening his eyes well, but there was no positive ptosis. The left globe oscillated somewhat from side to side. Many of his ocular muscles were paralysed. Thus, on the right side, the only muscle which enjoyed any material power was the inferior rectus. The others, although not absolutely paralysed, were almost so. On the left side the external rectus acted well, and the inferior fairly, but all the others only very slightly. When told to look upwards he simply converged his eyes. None of the muscles were absolutely paralysed, excepting, perhaps, the right external rectus.

The pupils dilated well with atropine. The optic discs were both of them pale, the left being much whiter than the other. The branches of the artery were but little diminished.

M. was by occupation a dock labourer, but he said that he often did not work more than two days a week, as he was lame in consequence of a compound fracture of one leg. He had been well fed and had a good appetite. He had always been temperate in stimulants, and had never smoked. His smell, taste, and hearing were perfect. He denied having ever had syphilis, or that he had experienced any sexual

failure, but in these matters he was not communicative. He had married, for the first time, eight years before I saw him. His wife was older than himself.

In March of same year he was in much the same state. His elub doctor had certified that he was insane; but his wife denied it, and although irritable, he had always to me appeared rational.

CASE 5.—The case of F. W. M—¹ is a very characteristic one. He was an engine-fitter, æt. 39, in apparently good health, and free from symptoms of spinal disease. He had suffered from complete syphilis twenty-two years before the eye symptoms began. The latter, although ultimately symmetrical, were not so at first. In the beginning all the recti on the left side failed, the pupil became dilated and fixed, and accommodation was lost. Amaurosis on the same side followed, and two years later similar symptoms were developed on the right side, and in the same order. Under a prolonged but perhaps too mild anti-syphilitic treatment his disease advanced very slowly. Six years after his admission, and nine from the commencement of his symptoms, he was quite blind, and his eyes almost fixed. He still possessed, however, slight power over certain of the recti. Excepting some occasional shooting pains in the head, and a degree of numbness of the forehead, he had not experienced any other nerve-symptoms, and was when last seen in good general health.

CASE 6.—The case of John H—² a labourer in wine-vaults, æt. 34 is of special interest, because symptoms of locomotor ataxy preceded those of ophthalmoplegia. It was not certain that he had ever had syphilis, and excess in venery, alcohol, and tobacco were amongst the possible causes. Obstinate constipation, retention of urine, impotence, and weakness of the lower extremities, were his earliest symptoms. Then followed symmetrical amaurosis and paralysis of the recti muscles on both sides. The pupils

¹ Detailed notes of this case extending over nearly six years were appended to the paper.

² Notes of this case also accompanied the paper.

had become dilated and motionless before the ocular muscles failed. He ultimately became almost blind.

CASE 7.—The next case which I shall mention is the last which has come under my notice, a very definite example, and one in which I had the pleasure of producing the patient for the examination of the Society on a former occasion. The patient is a butler in a country house. He is thirty-four years of age, and about ten years ago he went through a sharp attack of syphilis. He has now been for three years or more the subject of ophthalmoplegia externa in combination with some symptoms of ataxia. I have had him under treatment for about a year, and under specifics in full doses he has greatly improved, but the movements of his eyeballs are still in all directions much restricted. None of the muscles are absolutely paralysed, but all are very feeble. It is important to note that his pupils act fairly, and that his accommodation is perfect. His lower extremities were, at one time, very weak, and liable to much aching pain. He walked badly, and used occasionally to fall. These symptoms have now passed off, and he walks well, and appears to be in excellent health. This patient had taken the iodide before I saw him, and there is every reason to believe that it has been the means of saving his life, for, at one time, the symptoms were rapidly progressive, and he was confined to bed.

CASE 8.—Mr. Ilott, of the Bow Workhouse Infirmary, sent me in 1878 an interesting example of this malady in connection with a syphilitic history, and with modified symptoms of locomotor ataxy. The patient was an actor, æt. 30, of much intelligence, who had already been under hospital treatment, and who told me spontaneously that he had suffered from syphilitic disease of the brain. He had had syphilis eight years before, and at the time that his nerve symptoms began he was under Mr. World's treatment for a tertiary ulcer on the leg. The onset of his symptoms was, according to his account, sudden. He one day felt a "click" in his head and fell down in the street. He got up,

walked to an omnibus, and went home, but ever afterwards one eyelid drooped; a few weeks later the other eyelid drooped, and for a time both eyes were closed. Under treatment by iodide of potassium, extending over a year, he recovered, and when he came to me he considered himself well, and was not taking medicine. I found his pupils motionless, and of medium size, but accommodation almost perfect in both. His lids drooped to a moderate degree, and, excepting the internal rectus on each side, almost all his ocular muscles were more or less paralysed. The conditions were almost symmetrical. He could see well, and suffered no pain. His gait was decidedly ataxic, and he complained that his legs were weak. The sexual function was almost wholly in abeyance.

The subject of the above case was presented before this Society last year, and a week afterwards Dr. Gowers was kind enough to send to me for examination the subject of the following as an example of similar disease.

CASE 9.—A servant girl named B—, æt. 27, had been under Dr. Gowers' treatment for several years. Four or five years ago, after much headache, both eyelids began to droop, and subsequently the ocular muscles on both sides became involved. Her pupils continued to act well, and she retained sight and accommodation. At the time of my examination almost all the muscles were defective, and some almost completely paralysed. The inferior recti and the inner rectus on right side had most nearly escaped. There were some indications of ataxy. Her lower limbs were weak, and she walked badly, complaining that she could not go up stairs easily, and that sometimes her legs seemed to give way. Once she had tripped and fallen. I did not ask direct questions as to syphilis, and there was no other clue to it.

I am indebted to my colleague, Mr. Waren Tay, for the notes of two cases in which young children were the subjects of symmetrical paralysis of the muscles of the eyeball. In neither of them was there any proof of inherited syphilis, but in one the paralysis passed away under treatment by specifics in a manner which was very suggestive.

CASE 10.—A female infant, *æt.* 9 months, under care in 1876, at the London Hospital, had symmetrical ptosis and almost complete paralysis of all the ocular muscles. The lids drooped so as almost to cover the eyes. It was very difficult in so young a child to test each muscle, but so far as could be ascertained the paralysis was almost absolute, with the exception that the left eye moved a little both outwards and inwards. The condition had existed for fourteen days. The child was pale and fretful, but showed no signs of syphilis. There was an elder child, *æt.* 3, also healthy, but both of them had suffered from “thrush.” Iodide of potassium and mercury were ordered, and at the end of four months all trace of the paralysis had disappeared, and the child was cheerful and in good health. Five months after treatment was laid aside the child was seen again, and continued quite well. Two months after this, however, it was taken ill again, wasted, had convulsions, and died, but no information of this illness was given to Mr. Tay until some time after the death.

CASE 11.—The subject of Mr. Tay's second case was a girl, *æt.* 3, named A—, under care in March, 1878. Both eyelids drooped considerably, and several of the recti on both sides were paralysed. With the left eye she could look outwards but not in other directions, and with the right inwards and outwards fairly, but not in other directions. The pupils were of medium size, and sluggish. The symptoms had been coming on for six week, and there was a history of a slight fall. The child was fretful, but there were no indications of syphilis. A boy two years older than the patient was quite healthy. Iodide was ordered. Death occurred on April 19th, 1878, but no details of the latest symptoms are forthcoming, nor was a post-mortem obtained.

CASE 12.—I have met with one instance of this malady which was unquestionably in connection with inherited syphilis.

Abraham F— is at present unable to use any of the recti

muscles excepting to a slight degree, and his eyes are almost fixed in a divergent position. He is quite blind, and both discs are in an advanced state of atrophy. He has been at different times during the last few years under many of the ophthalmic surgeons of London, and also under Dr. Hughlings Jackson, by whom he was sent to me. He is florid and healthy-looking, but with a suspicious forehead, and quite characteristic teeth.

At the age of 13 his sight began to fail, and squint was noticed, and from this time his symptoms steadily advanced. For two years and a half he has been, as he is now, totally blind, with fixed eyeballs, and slightly drooping lids. There has been no evidence of increasing disease of the centres, and he still walks well, and has perfect use of all his faculties. He sleeps and eats well, and goes regularly to a blind school, where he reads by touch. He has now no headache, but in the earlier stages he had much headache and occasional "shiverings." His age is now 16.

His right eye is usually on a lower level than the left. He can only by great difficulty lift his lids so as to expose the whole of the cornea, and when at rest the corneæ are half covered, and he looks as if just going to sleep. Both eyes diverge. Both inferior recti are wholly paralysed, and both superior almost wholly so, the left being rather better than the right. The internal rectus of the right side is quite paralysed, and that of the left nearly so. The external rectus of the right is very feeble, but that of the left still acts moderately, being much the most active muscle on either side. All the obliques are paralysed.

CASE 13.—*Symmetrical and almost complete ophthalmoplegia externa without implication of the iris or ciliary muscle, but with defect of all muscles supplied by the fifth, and, in slight degree, of those by the seventh. No history of syphilis.*

A young man named George S— came to Moorfields in October, 1877, and was admitted under Mr. Wearn Tay's care. He showed no signs of inherited syphilis. He was only 19, and entirely denied any history of acquired taint.

He was from Devonshire and of healthy family. In addition to investigations by Mr. Tay and myself, his symptoms were carefully studied by Dr. Hughlings Jackson, Dr. Barlow, and others. On both sides his eyelids drooped so as to almost cover the eye, and all the muscles of the eyeballs were exceedingly feeble. His pupils, however, acted well, and accommodation was perfect (entire absence of ophthalmoplegia interna, and probable integrity of the lenticular ganglion). The ocular muscles were somewhat more completely paralysed on the right side than the left, and the lid drooped more, but on both sides the degree of weakness was almost complete, and involved all the muscles. In addition to the muscles of the eye, all those supplied by the fifth nerve were on both sides defective, but their paralysis was not nearly complete, and on both the muscles supplied by the facial were also slightly weak. There was no defect of sensation, nor any symptoms referable to the spinal cord. The vision and other special senses were perfect, and the man appeared to be in good general health. There had never been much headache.

The symptoms above mentioned had been present six months, and were supposed to be consequent on a trivial blow from a piece of wood on the left temple. There had been an interval of three weeks between the blow and the first drooping of the eyelids, and the blow itself was so slight that it may be reasonably doubted whether it had any connection with the paralysis.

Through the kindness of Mr. Fernie, of Barnstaple, under whose care the man is, I am enabled to state that at the present date, fifteen months since he was under care at Moorfields, the symptoms remain without advance, but with some improvement. Treatment by iodide of potassium has been pursued, but not regularly, nor in large doses.

I may suitably place in juxtaposition with this case one in which, as in it, the fifth-nerve muscles were involved in association with those of the eyeball. It differs, however, definitely from the preceding one in that there can be no doubt that the patient is the subject of syphilitic taint. It is a very

remarkable case, but I must state its facts as briefly as possible.

CASE 14.—Madame de T— æt. about 30 years, and in good bodily health. She has notched teeth, and has suffered a severe attack of interstitial keratitis. A sister, like herself, is the subject of inherited syphilis. Before, however, we confidently attribute her nerve-symptoms to inherited taint, I am bound to state that her husband believes that she has had acquired disease as well. About this no certainty is attainable. She is at present the subject of paralysis of all the ocular muscles on the left side, with only slight weakness of these on the right, but with double paralysis of the fifth, both motor and sensory, and some defect of both facials. She was formerly hemiplegic in her right limbs, but this has passed off. The account of the development of her symptoms is obscure, but the paralysis of the cranial nerves preceded the “fit.” She has already derived much benefit from treatment by large doses of iodide of potassium. The non-symmetry of the ophthalmoplegia, and the occurrence of hemiplegia, separate this case somewhat from the group under our consideration.

I have seen double paralysis of the ocular muscles once—and only once—in association with a history of apoplexy, and the case, although probably not very closely cognate to the others, must be briefly mentioned.

CASE 15.—Abraham B—, a healthy-looking old man, æt. 72, came under care in 1874, having, on the right side, paralysis of all the muscles of the eyeball, except the internal rectus. The condition was symmetrical, but on the left side the degree of paralysis was not so great. The eyeballs were converged, so as to be almost buried in the inner canthi. Accommodation was lost, but vision almost perfect. He was partially hemiplegic in the left limbs, and the muscles supplied by the right facial were weak. The history was, that the symptoms had followed a fit which had occurred suddenly three or four years ago. There was no history of syphilis, but its possibility was not denied. His vision failed

during the period that he was under observation, but in other respects his symptoms were stationary. It is difficult on any hypothesis of hæmorrhagic disorganisation to explain such a group of symptoms, and it is, after all, quite possible that the apoplexy was an accidental concomitant, and that the ophthalmoplegia was of the same nature as in the other cases.

P.S.—Since my paper was sent in I have found in my note-books two cases of this affection which I have omitted to mention.

CASE 16.—The subject of the first is a married lady whom I attended with Dr. Hughlings Jackson some years ago. She had double ptosis, with incomplete paralysis of most of the ocular muscles, fixed pupils, and loss of accommodation. She had been liable, previously, to most violent attacks of vomiting ("abdominal crises"). She improved very much under specifics. She became subsequently hemiplegic. She is still living (ten years after the first symptoms), but is confined to her room, and many of her ocular muscles still paralysed. There is much reason to suspect syphilis.

CASE 17.—The subject of the second is a sailor, who had syphilis six years ago, and in whom both-sided paralysis of the eye-muscles with ptosis set in rather suddenly after two months' premonitory symptoms in the form of failing accommodation.

I have detailed notes of both these cases, but must not further trespass on the patience of the Society. It will be seen that these add two more to the list of those in whom the disease appears to be due to syphilis.

Comments on the Series.

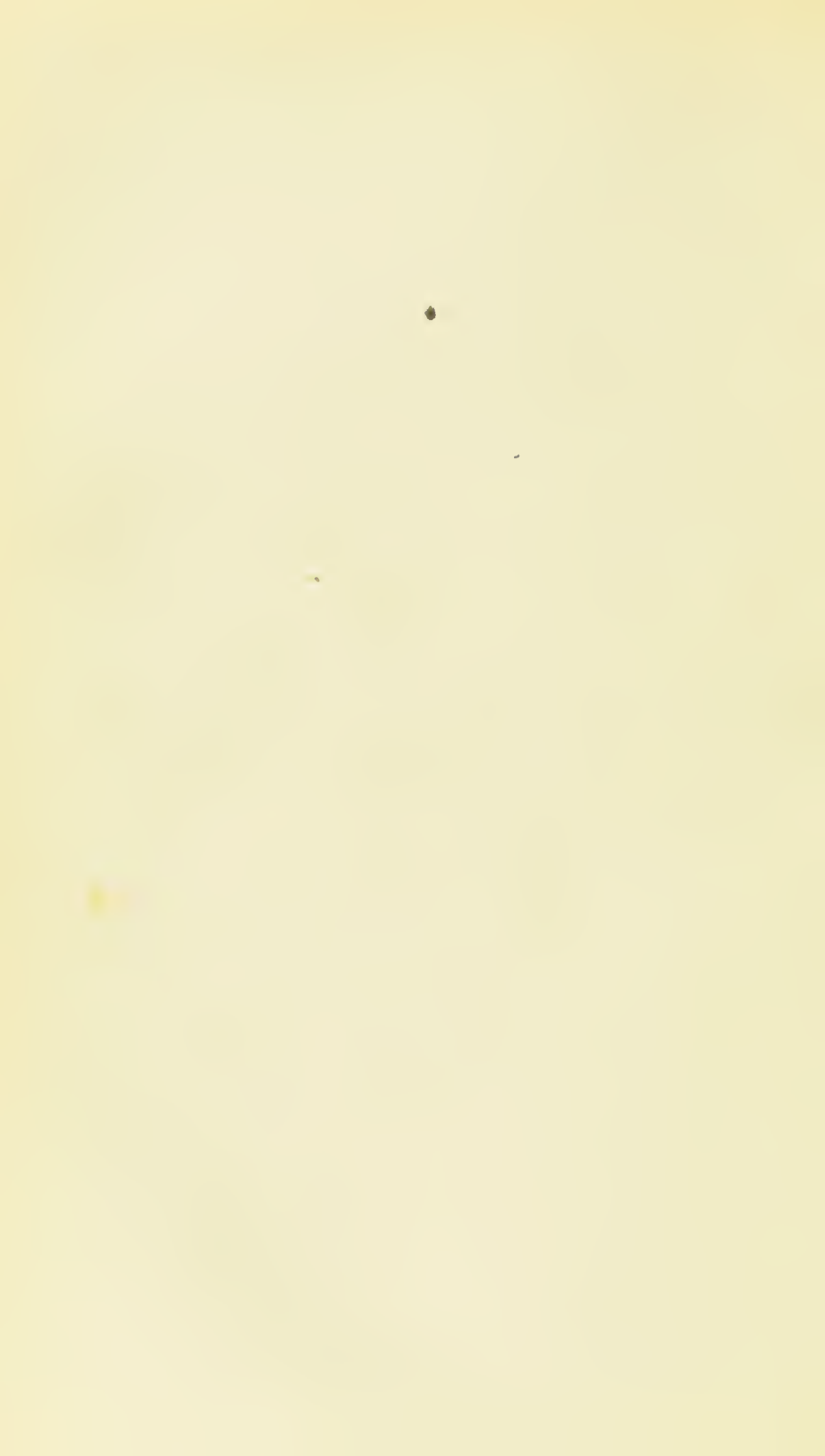
The facts are too few to permit of statistical analysis, but a brief summary may, perhaps, be useful. Of the fifteen cases four only occurred in females, and two of these were young children. One of the males was a boy the subject of inherited taint, and the remaining ten were all adults. In eight of the fifteen it seemed certain that syphilis was the

cause, in six acquired, and in two inherited. Of the remaining seven it may be said that a reasonable suspicion of syphilis might be entertained in several. Had it not been for an almost accidental revelation of the truth in the case of the man S— after prolonged fruitless investigation, I should certainly have asked the Society to believe that his was an example of the disease without any probability of syphilis. With such a fact in mind, one feels that it is almost impossible to make the negative even fairly probable. In Case 9 no direct question was asked, the patient being a single woman. In the two young children, although nothing could be proved, there were some suspicious facts, and one recovered under the iodide. The case in which evidence is most conspicuously absent is No. 13, but in this the patient is a young man, who may have denied the true history, or who may have either inherited a taint or had the acquired disease in some irregular way, of which he knew nothing. On the whole, the evidence which connects this affection with syphilis is exceedingly strong, and that which favours the belief that it can occur independently of it, must be held to be open to some doubt.

In five of the patients (all but one, known to be subjects of syphilis), the optic nerves were affected, and blindness, with white atrophy, resulted. In two cases the fifth nerves were symmetrically affected, and in two there was slight affection of the facials. The almost constant escape of the facials must be held to be a remarkable fact. In one case the palate was affected, and in one, the same patient, smell was lost. None of the patients were deaf and none had lost taste, and in only one was there any material anæsthesia of the skin of the face. In six cases the lower extremities were more or less weak, and liable to pain, the condition approaching more or less closely to locomotor ataxy. I much regret that, owing to imperfect knowledge on my part at the time of their occurrence, the details of facts in reference to this disease are often incomplete. There can, however, be no doubt that ophthalmoplegia externa is sometimes a part of the general malady known as progressive locomotor ataxy,

especially when that disease is due to syphilis. One of the patients became insane, and another was before death liable to attacks of violent mental excitement. Four of the fifteen are known to be dead, but of these a post mortem was obtained in only one. In several cases, owing either to the blindness or the youth of the patient, it was impossible to estimate the state of the accommodation, but in a certain number it was proved to be perfect, in a few it was absent, and in a few it was impaired. The pupil, never contracted, was almost always sluggish and of medium dilatation. In one well-marked case it acted fairly. In no single case was it very widely dilated. From these facts we may infer that the lenticular ganglion is often free from disease, and that the vaso-motor filaments, although often enfeebled, are not usually paralysed.

It is difficult to make any confident statements as to the progressive tendencies of the malady of which external ophthalmoplegia is a symptom. It is very definitely influenced for good by treatment, and in nearly every case specific measures were adopted. We may conjecture, however, from what happened in several, that in most instances it is an aggressive malady, and would end fatally if treatment were not resorted to. The effects of remedies in several of the cases were very remarkable, the patient being rescued from a very dangerous condition. At the time that most of the cases were under treatment my opinions, as regards the nature of the malady, were far less clear than at present, and hence a hesitancy in treatment, which was probably often prejudicial. The patients were benefited up to a certain point, but relapses occurred, and the remedy was not pushed sufficiently. It would seem that iodide of potassium is by far the best means of treatment, and that it ought to be given over very long periods, and in increasing doses without any limit as to precise quantity, excepting its effect on the symptoms. Although relapses are common, yet in several of the cases narrated a complete arrest appears to have occurred, no treatment having been resorted to for several years. In none, however, was the recovery complete.



A SUCCESSFUL CASE
OF
ABDOMINAL SECTION
FOR
INTUSSUSCEPTION;
WITH REMARKS ON THIS AND OTHER METHODS OF
TREATMENT.

BY
JONATHAN HUTCHINSON, F.R.C.S.,
SENIOR SURGEON TO THE LONDON HOSPITAL; SURGEON TO MOORFIELDS
OPHTHALMIC HOSPITAL AND TO THE HOSPITAL FOR SKIN
DISEASES.

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1874.

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THE case of intussusception which I am about to describe came under my care at the London Hospital in 1871.

The patient was a somewhat delicate female child aged two years. She had previously been seen by my colleague Mr. Waren Tay, who had diagnosed her disease, and by whom she was transferred to my care in order that she might be admitted as an in-patient.

From her anus there protruded a portion of bowel about two inches long, deeply congested and much swollen. By the side of this the finger could be passed, its full length, into the rectum without reaching the point at which the intussusception began. On carefully examining the extremity of the protruded part, I noticed that it did not present merely a rounded opening as usual in such cases. I was able easily to identify the pouch and valve of the cæcum, with the

opening into the ileum. Of these parts it was of course the mucous membrane which was visible, and the appendix cæci was wholly concealed between the folds of the intussusception. This discovery rendered it evident that we had to deal with an involution of bowel of very unusual length, which commencing at the cæcum had allowed the ileum to pass through the entire length of the colon, and actually to become extruded at the anus.

On examination of the child's abdomen externally the tract of bowel involved could be felt like a long firm sausage passing down the left side.

The mother of the child gave us the history that the latter had begun to suffer from pains in the abdomen, rather suddenly, about a month previously. Her first attack of pain was one Sunday afternoon, and was such as to cause screaming. It was quickly followed by a motion which contained blood, and by frequent vomiting. A fortnight after this, the child having been ailing the whole time, a protrusion of bowel was noticed at the anus. This was reduced by the surgeon then in attendance, and a cork pad was fitted over it. It was found impossible, however, to prevent the prolapse from recurring, and the child continued to be sick and to pass blood-stained mucus.

Three days before admission the prolapse increased to such a size that the parents were unable to reduce it, and were obliged on three occasions to call in surgical aid for that purpose. There had been no real obstruction of the bowels, but only temporary constipation at times.

The child, at the time of her admission, looked very ill. Her countenance was pale and anxious, and from her mother's description it was evident that her strength had been failing rapidly during the last few days. It appeared that she was almost constantly engaged in straining to get rid of the bowel which filled the rectum.

Our first measure of treatment consisted in putting the child under chloroform, and then, whilst she was held up by the feet, distending the rectum to the utmost with warm water.

By this means the involuted part could be forced up into the abdomen so as to be quite out of reach of the finger, and once or twice I tried to hope that reduction had been effected. On each occasion, however, when the lower bowel was allowed to empty itself, the intussuscepted part became prolapsed as before, and showed clearly that we had gained nothing.

My experience of several other somewhat similar cases, all of which had resulted in death, after patient and repeated attempts by the injection plan, did not encourage me to expect success in this.

It was very evident, from the child's condition, that unless relief were afforded she would not live long, and I therefore felt justified in telling the parents that although an operation would be, in itself, very dangerous, yet I thought that it afforded the only chance.

They begged me to give the child the chance if I thought it was one, and we accordingly determined to lose no time.

The child having been taken up into the operation theatre, chloroform was again administered, and I then opened the abdomen in the median line below the umbilicus, and to an extent admitting of the easy introduction of two or three fingers. I now very readily drew out, at the wound, the intussuscepted mass, which was about six inches long. I found that the serous surfaces did not adhere, and that there was no difficulty whatever in drawing the intussuscepted part out of that into which it had passed. Just as the reduction was finished the appendix cæci made its appearance, confirming the opinion which had been formed as to the precise part of the bowel involved. The opposed serous surfaces did not present a single flake of lymph, and they were congested in only a moderate degree.

Having completed the reduction, I put the bowel back into the abdomen, and closed the wound with harelip pins and interrupted sutures.

The operation had been an extremely simple one, and had not occupied more than two or three minutes.

The abdomen having been well supported by strapping,

cotton wool, and a flannel bandage, the child was returned to bed.

The after-treatment consisted in the use of milk enemata every three hours, with the occasional addition of five minims of tincture of opium.

No vomiting occurred after the operation. No food whatever was allowed to be taken by the mouth during the next two days. The temperature, on the evening of the operation, was 100.5° , but subsequently fell to 99° , and with the exception of the fifth day, on the evening of which it rose to 101.7° , it never exceeded 100° . Chloroform was administered on two or three occasions to allow of the wound being dressed without the child's screaming. The pins were taken out on the fourth day, that is, seventy-two hours after the operation.

I had felt much anxiety as to the healing of the abdominal wound on account of the thinness of the parietes, but nothing untoward occurred.

The child recovered without having ever showed the slightest symptom of peritonitis, and left the hospital in excellent health about three weeks after the operation.

Nothing but fluid food (milk and beef tea) had been allowed through the greater part of this time from a fear of producing any return of the intussusception. The child was fitted with an abdominal support when discharged, but the scar was sound and strong, and there was no tendency to bulging.

The successful issue of a single case goes but a very little way towards proof that the line of practice adopted was the proper one. I purpose, therefore, with the permission of the Society, to discuss this question in some detail; and the first items of evidence which I will mention are the cases which had previously come under my own observation.

About twelve years ago I operated for harelip upon a delicate child about ten months old. The child had been brought from Somersetshire on purpose to have the operation done, otherwise its feeble state of health would have caused

me to defer it. The lip healed well, but about the tenth day the child began to pass blood and slime. On examination *per anum* I found a long intussusception occupying the rectum. It never protruded at the anus. I tried during six days a great variety of means with a view to the replacement of the involution. Chloroform was repeatedly given. Injections of air and of water were made over and over again, and in various positions of the body, and attempts were also made with a long tube to push the bowel into place. Several times I thought I had succeeded, and on one occasion the passage of a considerable quantity of fæces made me feel confident that reduction had been effected. About six hours after this occurred, however, the child, who had been gradually failing in strength, died. No post-mortem was permitted, and I am unable to state whether the reduction was complete. My colleague, Mr. John Adams, had, on one occasion, seen the patient in consultation with me, and had assisted in attempts at reduction.

A few years later I saw another similar case in consultation with Dr. James and Dr. Bright, at Forest Hill. Our patient was a remarkably fine healthy boy, of about three years of age. A long intussusception occupied his rectum, and came low down, although it never actually protruded. Again we tried every plan that we could think of, but without success. Enemata were used in the most forcible manner with the child's body inverted, and they were repeated several times a day, and often under the influence of chloroform. Bougies of various kinds were also carefully tried.

The child sank from exhaustion about a fortnight after the commencement of the symptoms.

Dr. James obtained a post-mortem which confirmed the diagnosis as to the condition of parts.

A third case was brought under my notice by Mr. Warren Tay three years ago. Its subject was a female child aged about fifteen months. Blood had been noticed escaping from the bowel on the day before Mr. Tay was consulted. The child had great pain and was repeatedly sick. On

examination of the bowel, Mr. Tay discovered at the distance of two or three inches from the anus the extremity of a long intussusception. The portion of bowel involved could also be easily felt through the abdominal wall. Repeated attempts were made by manipulation and by injections to effect replacement of the bowel, but without success, and on the seventh day from the commencement of the symptoms the child died. The autopsy showed an intussusception of the transverse into the descending colon, involving, however, only about two inches of the bowel. It was thought probable that the greater portion had been reduced. The small intestines were much distended, and were somewhat congested on their peritoneal surface, but were quite free from lymph. There were no adhesions whatever of the opposed peritoneal surfaces of the intussuscepted part, and it could have been reduced by traction with the greatest ease.

The particulars of a fourth case, which occurred in a young adult man, have been recorded by me in vol. vii, p. 193, of the 'Pathological Society's Transactions.' In this case the patient lived four months from the commencement of his symptoms, and at the post-mortem six inches of the ileum, the entire cæcum, and first part of the ascending colon, were found invaginated within the latter. The coats of the bowel were much thickened, but there were no adhesions, and reduction by traction from within would have been quite practicable.

My experience does not afford a single case at all to be compared with the preceding, in which the patient survived. I treated successfully, by means of injections under chloroform, a case in which I had conjectured that intussusception was present, but the diagnosis did not rest on any certain data, and the stage was a very early one. The patient was a delicate little boy. He had been sick, and had passed slime and blood. I thought that I could feel through the abdominal wall a lump very much like that caused by an intussusception, but there was nothing to be

felt by the rectum. After a free injection under chloroform the bowels acted and the child recovered.

In another case I had a good deal of trouble with a short intussusception about five inches from the anus, which had resulted from the too rapid reduction of an ordinary prolapse of the rectum, seven or eight inches in length. In this instance, after a considerable manipulation, I was successful in effecting a complete reduction.

Thus, it will be seen that at the time the case which is the subject of this paper came under my care my own experience did not supply a single one at all parallel to it in which the patient had been saved; whilst in three all endeavours had resulted in disappointment. In fact, such had been the impression which these cases had made upon my mind, that I had quite determined to resort to operation when next any similar one should present itself.

The case which I have brought before the Society is, so far as I am aware, the first successful one of its kind in English practice. The operation itself, however, is by no means a novelty, and at least three examples of its successful performance are on record.

I may be permitted briefly to refer to the particulars of these.

In one recorded by Velse, and quoted by M. Hévin, in the 'Memoirs of the Royal Academy of Surgery of Paris,' 1784, the patient was a woman aged 50. Intussusception was diagnosed by Nuck, at whose suggestion the operation was performed. The incision was made on the left side of the abdomen, four fingers' breadth from the umbilicus. The intestine was drawn out, and the intussusception was liberated without difficulty, as no adhesions were encountered. The wound was closed and the patient recovered, and lived for twenty years afterwards. In the performance of this operation the intestines were fomented with tepid milk, and the intussuscepted part was well oiled. It is spoken of as having been very easy of performance.

The next case occurred in 1825, and is recorded by Dr. Fuchsius, of Olpe, in 'Hufeland's Journal' (quoted in the

'Edinburgh Medical and Surgical Journal,' July, 1825. Its subject was a man aged 68, who was seen on the sixth day of his illness. In the neighbourhood of the navel, rather on the right side, there was evident hardening and tenderness, which increased and somewhat changed position during attacks of spasm. After five days of further treatment by clysters, &c., the abdomen was opened. An incision was made on the outer edge of the right rectus two inches above the navel. The intussusception was soon found. There were no adhesions, but such difficulty was encountered in effecting reduction that the surgeon decided to open the intestine. This was done by an incision two inches long, admitting of the introduction of the fingers into the intussuscepted part. Reduction was then accomplished, about two feet of bowel being disengaged. The wound in the intestine was stitched up. The patient recovered. The operator recommends, I have no doubt very judiciously, that in future operations the incision should be made in the linea alba, and that, if it be necessary to put stitches in the intestine, they should be cut close off instead of being left with a long end, to come out at the abdominal wound.

A third case of recovery occurred in the practice of an American surgeon, Dr. Wilson, and is recorded in the 'American Journal of the Medical Sciences' for 1836. The patient was a negro aged 20, and the intussusception had lasted seventeen days. There were adhesions, and great difficulty was encountered.

In British practice the operation appears to have been performed only once, and then under very unfavorable circumstances. The patient was an infant only four months old, in whose case Mr. Spencer Wells was consulted, on the fourth day of an intussusception with acute symptoms. The diagnosis was positive, for the involuted portion of intestine could be reached by the finger in the rectum. It was not till the fifth day, when the patient was almost dying, that the parents of the child consented to the operation. The abdomen was opened in the middle line below the umbilicus. The intussuscepted portion was easily found,

but the constriction was so tight that it was not without great difficulty that it was reduced. Its release was at length accomplished, the intestines returned, and the wound closed. The bowel above the constriction being greatly distended with flatus some needle punctures were made for its relief. The child died about five hours after the operation.

As regards other fatal cases after operation, as already implied, I have not been able to find any in English records. Several continental writers refer vaguely to such, and some speak of them as if they had been numerous. I have found a case reported by Carrier, of which the following are the particulars (as given in 'Virehow's Jahresbericht'). The patient was a man aged 23. Pain came on suddenly, and a tumour could be felt in the ileo-cæcal region. On the fifteenth day the abdomen was opened, and an attempt was made to extricate an intussusception which was discovered, but the attempt was unsuccessful. The small intestine higher up was therefore opened. The patient died seven hours afterwards. The post-mortem showed an intussusception of the ileum into the cæcum.

Fatal cases have also been recorded by Max Hertz, Pirogoff, and Gerson. In two of these great difficulties were encountered in freeing the intussuscepted part, and in Pirogoff's case it was found impracticable.

Before attempting further to discuss the propriety or otherwise of this operation I may suitably refer to the symptoms which characterise intussusception, and to some of its natural terminations.

There is a class of cases, and, perhaps, not a very small one, of which the one I have recorded is an example, in which all obscurity as to diagnosis is removed by the discovery of the intussuscepted bowel in the rectum. In all suspected cases this examination should be made. It is quite evident from the descriptions given of the post-mortems in many cases that had the bowel been sought for by the anus it could have been felt. The symptom next in value,

and, indeed, perhaps not second in real importance, is the manipulation of the abdomen and the discovery of the long or oval sausage-like mass which an intussusception constitutes. This is far more easily done than is generally thought possible, especially so with the aid of chloroform. Unless the parietes of the abdomen be fat my impression is that by firm pressure, the patient being under the full influence of an anæsthetic, all doubt as to the existence or non-existence of intussusception, and as to the completeness or incompleteness of its reduction, may usually be removed.

Amongst the other less important symptoms we must mention pain in the abdomen, attacks of spasms, the passage of bloody mucus or of pure blood by stool, the existence, in some cases, of obstruction of the bowels, and in some of almost constant desire to strain at stool. These symptoms will vary much in degree of severity in different cases, and it is of considerable practical importance to remark that the cases may be roughly grouped, much as we do those of hernia, by reference to the tightness of the constriction. We have cases of intussusception accompanied by *strangulation*, and we have others which are *irreducible* only. The former tend rapidly either to the death of the patient, or his relief by gangrene of the constricted part. Their duration is rarely more than a few days. Those, however, in which there is only an irreducible invagination without either stoppage of the contents of the tube or interruption in its blood-supply may run a prolonged course, and they have a greatly diminished chance of spontaneous cure by gangrene. It is in these latter that operative interference is most necessary and has the fairest chance of success. In these the patient may live on for weeks, and the surgeon is permitted a good opportunity both for establishing his diagnosis and proving the inutility of other measures of treatment. The patient's death when it at length arrives is brought about more by exhaustion from long-continued pain than from any inflammatory process. In this class of cases I believe it would seldom be found that the coats of the intestine had become adherent to each other, or that there was any material

difficulty in effecting reduction after opening the abdomen. If the operation were resorted to in cases of *acute strangulation* there would always be the risk that the surgeon might find the parts in a state of gangrene, and might discover that he had interfered only to take away the patient's last chance.

It seems, therefore, of great importance to insist that before attempting the operation the tightness of the strangulation should be estimated.

The diagnosis between mere irreducibility and tight strangulation will usually be easy. In the one there will be severe sickness, constipation, and great general distress tending to collapse, whilst in the other the bowels will continue to act, sickness will be almost wholly absent, and the patient may suffer comparatively little.

I cannot better illustrate this statement than by reminding the Society that in my own case the state of things had existed for a month, and that so slight had been the patient's general symptoms that a surgeon had ordered a cork pad to keep back what he supposed to be an ordinary prolapse.

The same mistake is mentioned as having occurred in several other cases on record.

If in a case of tight strangulation with severe symptoms the patient were seen early and quite before any indications of collapse had appeared, my impression is that opening the abdomen (insufflation, &c., having failed) would be safer than to leave the case to the chance of cure by gangrene, but if the stage were more advanced I think I should prefer to give opium and trust to nature's method.

Before finally deciding as to the need of surgical interference in that class of cases in which, as I have just shown, it is alike most hopeful and most necessary, we must ask what other chances of recovery are before the patient.

Given a case of intussuscepted bowel without sickness and without constipation, therefore, presumably without strangulation, what degree of probability is there that recovery may be obtained either by natural processes or by methods of treatment short of operation? I have just hinted that the

chances of gangrene are not great.¹ The constriction is not tight enough to cause it, and although it must be granted that in a few instances after the bowel has remained for considerable periods in a state of mere incarceration, gangrene does eventually occur, yet it is a rare event; much more commonly the patient sinks from exhaustion. If the chances of recovery by gangrene be but little my impression is that those by spontaneous return of the parts to their natural condition, or their reduction under treatment by insufflation, &c., are much less. At any rate the surgeon will soon know how much he has to hope in either of these directions. I have not found any case recorded in which spontaneous return of a well-recognised intussusception occurred, and those in which art succeeded are comparatively few. It is, of course, the surgeon's duty to give a patient trial to injections, to use fluids and air alternately, and to use them with the patient's body inverted and with the muscles set at complete rest by an anæsthetic, but if he should not succeed quickly by these means it is not likely that he will succeed at all.

¹ Nor must it be forgotten that even when gangrene occurs it does not necessarily lead to recovery. In several cases in the table appended to this paper death followed the expulsion of the detached portion. Dr. Hiltou Fagge, in an excellent paper in the 'Guy's Hospital Reports' for 1869, writes as follows upon this point: "Now, as we have already seen in ileo-cæcal intussusception 'expulsion' comparatively seldom occurs, and when it does occur it frequently only postpones the fatal termination instead of entirely preventing it. The patient dies some months afterwards from contraction of the cicatrix, which had formed at the seat of the disease. This appears to me to afford a weighty additional argument in favour of the attempt to explore and pull out an ileo-cæcal intussusception, when the case is directly diagnosed at an early stage, and when inflation has failed to overcome the disease." The precise cause of death suggested by Dr. Fagge is a very probable one, but there are others yet more frequent. A case under the care of M. Fanchon ended fatally three days after the expulsion, there being an abscess at the seat of disease. A case recorded by Dr. Baillic, in which a yard of colon had been passed, resulted in the death of the patient three weeks afterwards. In two other cases death occurred two and four weeks respectively after the sloughing. In another a post-mortem showed a cavity containing feces, which intervened between the two ends of the bowel; and in another, fever, vomiting, and diarrhœa, preceded death.

Nearly all the recorded instances of success were very recent cases or cases in which the intussusception was small. They serve but little to encourage the surgeon when he encounters such a case as that which I have just recorded. In very few, indeed, was the intussusception long enough to be felt in the rectum, and in scarcely any did success follow after several failures. The opinion of some of our best authorities is so definite on this point that they recommend that all attempts at replacement should be abandoned if they have not succeeded within a short period.

The literature of intussusception is very large, and it is not my intention to trouble the Society with any attempts at its statistical analysis.

My friend and colleague Mr. Waren Tay has, however, kindly collected for me references to a great number of cases, and from these I may be permitted to extract such facts as may seem to bear most definitely on the subject under discussion. We have confined our attention to cases in which the intussusception occurred in the lower bowel.

In the table appended to this report will be found the particulars of numerous cases in which the intussuscepted part presented into the rectum, and either was or might have been felt by the finger. This table must not be considered in any sense exhaustive, but it may yet furnish us with some valuable data.

Of these cases a very few ended in recovery *without gangrene*. In one of these an infant aged eighteen months, in whom an intussusception could be felt by the finger in the rectum, was treated early under the care of Dr. Steele by powerful injections of warm water, and had no return of the symptoms. In a second, a child, under the care of M. Cabaret, had prolapse of bowel from the anus twelve inches in length; whilst at the same time a sound could be passed up for some distance between the rectal mucous membrane and the invaginated parts. Reduction was effected by a gum elastic bougie, which was retained for several hours to prevent relapse. A similar measure was successful in the hands of Dr. Osborne, in a very similar case. It is to be noted that in

all these three cases it would appear to have been the lower part of the colon only which was involved, and it is obvious that in such the chance of success is far greater than when the cæcum or the small intestine comes down.

In the first of the cases of recovery *after gangrene* the patient was a boy aged 6, in whom the early symptoms had been those of strangulation. The bowel appeared at the anus, and about the eighth day a portion, twenty-three inches, came away. In the second case, again, we have symptoms of severe strangulation, and the patient, a girl aged 11, appeared to be at the point of death. As early as the fifth day a portion of colon, cæcum, and mesentery, measuring nearly fourteen inches, was detached.

In the third case the patient was a man aged 40, who voided twenty-eight inches of colon on the fourteenth day.

The fourth case is one of the most interesting on record, from the unusual length of the period before the bowel separated. The specimen was exhibited by Dr. Quain, before the Pathological Society, and the case is recorded in the tenth volume of its 'Transactions.' The patient, a boy aged 5, had suffered for four months from obscure abdominal symptoms, and was finally relieved by the escape of twelve inches of bowel including the cæcum, part of the ileum and part of the colon. He had had irregular constipation and some sickness, but at times his appetite had been voracious. He had never passed blood.

In the fatal cases the influence of early age in accelerating the event seems well marked, a large majority being infants under the age of one year, who died after periods of from one to three days' illness.

It is clear that if, in infants, operative interference is to be of any use it must be resorted to very early. Examination of the cases in which the patient was under two years of age shows that eleven died within two days, five lived as long as the sixth or seventh day, one to the twentieth, and a single very exceptional one survived for a period of nine weeks. This last case is published by Mr. Sidney Jones in the 'Pathological Society's Transactions.' In it the small intes-

tine had travelled through the entire length of the colon, and protruded at the anus until as much as six inches were visible. The child had free action of the bowels, took the breast well, and never vomited. In the first instance, however, severe symptoms of obstruction had been present. Death was finally caused by exhaustion from straining and by the slowly progressing gangrene of the extruded portion. Mr. Jones mentions in his account of the post-mortem a fact of very great importance in reference to the question of operation—that the serous surfaces of the opposed portions of bowel were adherent along their whole extent by firm, fibrous membrane.

In the absence of any data as to the manner in which operations of this kind are borne by very young children we shall probably be right in believing that they are far less hopeful than in those somewhat older. On the other hand it is our duty to remember that the cure by sphacelus, which occurs with tolerable frequency in others, is scarcely ever met with in infants, and that unless rectification is obtained by injections, without much delay, speedy death is almost certain to result.

Very valuable information might be furnished to the surgeon by post-mortem examination as to the feasibility of operative interference in these cases; unfortunately, however, but few of those who have published cases give us specific details on this point. During the last session of the Pathological Society, Dr. Edwards Crisp exhibited a specimen from a child aged eight weeks, with the statement that so tightly was the invaginated part enclosed that it would have been impossible to withdraw it. Mr. Sidney Jones in one case, as just mentioned, found the peritoneal surfaces universally and firmly adherent. In two cases of my own and in one of Mr. Waren Tay's it was found, at the post-mortem, that traction from within the abdomen easily reduced the invagination, and that there was no material damage to the coats of the bowel. In a very considerable number of published cases the details of the post-mortem warrant the belief that an operation would not have been difficult, since no mention

is made either of tightness of constriction, adhesions or gangrene.¹

One fact disclosed by post-mortem records I may ask especial attention to, and that is the almost uniform absence of peritonitis as a complication. This is specially noted in a great number of cases. In intussusception as in strangulated hernia, and other forms of abdominal obstruction, it may, I think, be taken as an established fact, that unless actual perforation has occurred there will be no peritonitis.

In conclusion, that I may not further weary the Society by the details of isolated facts, I may briefly record my conviction that any one who will carefully examine the evidence for and against will come to the conclusion that operations for the relief of intussusception are not only warrantable, but that in a large number of cases they are urgently demanded.

The cases most hopeful are those in which the symptoms denote incarceration rather than strangulation, and in them the surgeon may take the knife in hand with a good prospect that he will encounter no serious obstacle, and that he will not find either very tight constriction, adhesions, or gangrene. Of the other cases, there are many in which, if the patient be seen early, there is sufficient hope, notwithstanding the severity of the symptoms, to justify the operation, though the surgeon must expect in such to find occasionally that the conditions preclude its completion. Lastly, in a small minority, seen late, or in which the symptoms have from the first been extremely severe, it is probably wisest to

¹ I do not know that we shall gain much by citing the opinions of authors for or against this operation. Amongst many who dissuade us from it are, Dr. Brinton, Mr. Holmes, and Mr. Pollock. On the other side, MM. Rilliet and Barthéz, who base their opinion on post-mortem examinations, in which they found reduction very easy, state that "after employing medical treatment during three or four days, and after having made several attempts at inflation, we should not hesitate to perform gastrotomy." Drs. Meigs and Pepper, who quote the above passage, appear to be quite favorably disposed to the operation, and Dr. West's conclusion is to the same effect. Dr. Hilton Fagge, after a careful summing-up of evidence, is a decided advocate of the operation, but suggests it would be well for the surgeon to wait until a case comes before him which is known not to be already of long standing.

decline an operation and to trust to the chance of gangrene.

The following conclusions are appended by way of summary of the facts and statements contained in my paper.

Conclusions.

1. That it is by no means very uncommon for intussusception to begin at the ilio-cæcal valve, and to progress to such a length that the invaginated part is within reach from the anal orifice or even extruded.

2. That it is of great importance in all cases of suspected intussusception to examine carefully by the anus.

3. That in almost all cases of intussusception in children, and probably most in adults, the diagnosis may be made certain by handling the invaginated part through the abdominal wall.

4. That the prognosis of cases of intussusception varies much; first in ratio with the age of the patient; and, secondly, with the tightness of the constriction.

5. That in a large proportion of the cases in which children under one year are the patients, death must be expected within from one to six days from the commencement.

6. That in the fatal cases death is usually caused by shock or by collapse from irritation and not by peritonitis.

7. That in many cases it is easy, by estimating the severity of the symptoms (vomiting, constipation, &c.), to form an opinion as to whether the intestine is strangulated or simply irreducible.

8. That in cases of strangulated intussusception, whilst there is great risk of speedy death, there is also some hope that gangrene may be produced and spontaneous cure result.

9. That in cases in which the intussuscepted part is incarcerated and not strangulated, there is very little hope of the occurrence of gangrene, and it is probable that the patient

will die, after some weeks or months, worn out by irritation and pain.

10. That the chances of successful treatment, whether by the use of bougies or by the injection of air or water, are exceedingly small, excepting in quite recent cases, and that if the surgeon does not succeed by them promptly it is not likely that he will succeed at all.

11. That the cases best suited for operation are those which have persisted for some considerable time, and in which the intestine is only incarcerated, and that these cases are also precisely those least likely to be relieved by any other method.

12. That in the cases just referred to, after failure by injections, bougies, &c., an operation is to be strongly recommended.

13. That the records of post-mortems justify the belief that, in a considerable portion of the cases referred to, the surgeon will encounter no material difficulty in effecting reduction after opening the abdomen.

14. That the circumstances which might cause difficulty are, first, the tightness of the impaction of the parts; secondly, the existence of adhesions; and thirdly, the presence of gangrene.

15. That in selecting cases suitable for operation the surgeon should be guided by the severity of the symptoms, in his estimate of the tightness of the strangulation, and also as to the probability of gangrene having already set in.

16. That in cases in which the patient's symptoms are very severe, or the stage greatly advanced, it may be wiser to decline the operation and trust to the use of opiates.

17. That the operation is best performed by an incision in the median line below the umbilicus.

18. That in cases of intussusception in young infants (under one year of age) the prognosis is very desperate, scarcely any recovering excepting the few in whom injection treatment is immediately successful, whilst a large majority die very quickly.

19. That the fact just referred to may be held to justify,

in the case of young infants, very early resort to the operation.

20. That it is very desirable that all who in the future have the opportunity for post-mortem examination of intussuseption cases should give special attention to the question as to whether an operation would have been practicable, and should record their results.

TABULAR STATEMENT

Of the Results of different Plans of Treatment, &c., in Cases of Intussusception of or into the Lower Bowel.

THE following table has been compiled for me by Mr. Warren Tay, and comprises cases more or less closely similar to the one which is the subject of my paper. We have selected from various sources the recorded examples of intussuseption of the bowel, *in which the intestine passed low down into the colon*. We did not wish to include cases in which the small intestine alone was involved, since these, both as regards treatment, symptoms, and probable results, belong to a different category. It was necessary, therefore, to adopt some definite line of limitation, and this we have found in the presence or otherwise of the intussusepted part in the rectum. It is believed that no cases are included in the following list in which the bowel was not either discovered by the finger or, at any rate, might have been, had an efficient examination been made. It will be seen that this discovery of the bowel by the finger is a symptom of the utmost importance, since it places the diagnosis, both of the nature of the lesion and the part of bowel involved, beyond question.

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
1	Dr. Trevor, Am. Jour. Med. Sci., Jan., 1852, p. 277	12 years, M.	Occasional pain for two or three weeks, then severe pain followed by vomiting; tumour felt in left side of abdomen and on rectal examination; attempts at replacement with stomach tube, etc.	4 days, death	Intussusception of 12 inches of jejunum into the succeeding 12; the mass thickened by inflammation; there was no general peritonitis; the upper end was just below and within the arch and descending colon, and the lower end was firmly impacted in the pelvis; two smaller ones were found of 2 inches and 1 inch in length	Was diagnosed. This was small intestine, yet forced down nearly to anus.
2	Mr. E. Y. Steele, Lancet, 1849, vol. i, p. 680	8 mos., M.	Passage of blood; tumour felt in rectum; slight prolapse; enemas, etc., were without avail	2 or 3 days? death	No post-mortem obtained.	
3	Do.	4 mos., M.	Tumour felt in the rectum; various efforts at reduction made, but without avail	2 days, death	No general peritonitis; the lower end of the ileum, the cæcum, the ascending and greater part of the transverse colon, were invaginated into the sigmoid flexure; the upper two thirds of the innermost portion were of a claret colour, the lower third greenish brown, and in a state of complete sphacelus.	
4	Do., Lancet, 1859, March 19, p. 287	18 mos., F.	Tumour felt in the rectum; "a considerable length of invaginated gut was occupying the rectum;" there was a watery discharge tinged with blood; the case was treated early by power-	6 to 12 hours, recovery	There was no feeling perceptible as if the intussusception had suddenly given way, but the gut did not again hudge down into the rectum	An instance of success by encrusta.

Ther. Med. Chir., 13, 1858, Schmidt's Jahrbüch., 101, 1859, p. 322	M.	then prolapsus which could be replaced; on seventh day a sud- den prolapse, which could not be reduced; on the eighth day twelve inches of colon were pro- lapsed, while a sound could be passed a long way upwards by the side of it	recovery	fingers only were without avail; Le Pelletier's method was suc- cessful; pressure with a gum- elastic sound, which was not re- moved finally till the third day, as the prolapse recurred a few minutes after the pressure was withdrawn	probably the pro- lapse was of rectum chiefly. Compare with the first men- tioned, on page 39.
6 Dr. Worthington, Am. Journ. Med. Sciences, Jan., 1849, p. 97	34 years	For two years had had various intestinal symptoms; for six weeks had diarrhoea and protru- sion of bowel at the anus	6 weeks? death	The cæcum was invaginated along the whole length of the colon and rectum, carrying with it the lower portion of the ileum and the first part of the colon; more than 2 feet of bowel had been inverted; the cæcum must have passed through the sphinc- ter in the child's efforts to evacuate the bowels.	
7 Mr. T. Blizard, Med. Chir. Trans., vol. i, p. 169, 1812	5 mos., M.	A tumour about the size of an egg on the left side of the abdo- men	4 days, death	The lower end of the ileum, the cæcum and its appendix, the ascending colon, &c., were in- vaginated into the sigmoid flex- ure and rectum to within $1\frac{1}{2}$ inch of the anus; there was no peri- tonitis; the invaginated parts were gangrenous; "they might have separated had the child's constitution not given way."	
8 Mr. Langstaff, Edin. Med. Journ., July, 1807, p. 263	3 mos., ?	Vomiting; passage of blood and mucus; hard tumour felt on the left side of the abdomen; pro- lapsus	5 days, death	The end of ileum, the cæcum, and colon invaginated into rec- tum; there was also a smaller invagination in the opposite di- rection.	

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
9	Penquier, L'Union Médicale, Ang. 22, 1861; Dr. Smith, Amer. Journ. Med. Sci., Jan., 1862	4 mos., M.	Passage of blood; tumour felt five or six inches from the anus, and a hard elongated tumour in the left iliac fossa	2 days, death	Cæcum and its appendix, ascending and transverse colon into descending colon, close to rectum. It is stated that invagination of the large intestine is common among children in Brittany, where the practice prevails of bandaging them tightly in linen and leaving them alone for some hours.	
10	Dr. Carter, Lancet, June 9, 1849, p. 607	4 mos., F.	A large sausage-like body felt in left iliac region; enemata only reached a certain point	6 days, death	Invagination found in left iliac region, but not described	Probably could have been felt per anum.
11	Mr. Nind, Lancet, 1849, vol. i, p. 681	4 mos., M.	One sanguineous stool	4 days, death	Cæcum and ascending colon in sigmoid flexure. "The invagination was so complete that from the congestion, &c., which had occurred I could not reduce it till the enclosing portion of the gut was divided nearly in its whole length;" it "was of a deep purplish colour, with small ash-coloured patches of gangrene."	Ditto.
12	Dr. Edwards, Med. Times & Gaz., 1861, vol. ii, p. 531	3½ years, M.	Pain and swelling of right side of abdomen, which passed away eight months previously; prolapse, which could be returned four months previously; for two days the prolapse became permanent	4 days? death	<i>Post-mortem</i> .—A portion of bowel 2½ inches in length protruded from the anus; a large mass could be felt in the left side of the abdomen; the cæcum and colon invaginated into descending colon, sigmoid flexure and rectum. The part protruding	

13	Mr. Ash, Med. Times & Gaz., 1867, vol. ii, p. 505; Brit. Med. Journ., 1868, vol. i, p. 117	6 years, F.	A tumour could be felt within the anus, and finally a prolapse occurred; attempts to push the tumour back unsuccessful	10 days, death	<p>from the anus consisted of an inverted cæcum and its appendix, and the opening of the ileo-cæcal valve was visible on the prolapsed part.</p> <p>A post-mortem was obtained, but no description of the state of parts is given; it is called an intussusception of the rectum.</p>
14	Mr. Young, Brit. Med. Journ., vol. ii, p. 779, 1859	9 mos., M.	A tumour could be seen and felt less than an inch from the anus	28 hours, death	<p><i>Post-mortem.</i>—Ileum, cæcum, and colon invaginated into colon and rectum.</p>
15	Dr. Philipson, Brit. Med. Journ., Sept. 24, 1864	10 mos., M.	Tumour felt about four inches from the anus; repeated attempts at reduction (digital, enemata, and insufflation) unsuccessful	28 hours, death	<p>Ileum, cæcum, and colon, invaginated into colon and rectum, the lowest portion of the tumour being the part near the ileo-cæcal valve. "An attempt was made to reduce the tumour in the same manner as during life, but was quite unsuccessful. Traction was then made on the small intestine, and it was only dislodged after a considerable amount of force." "The cæcum and appendix were then drawn out, but still more force was required; the adhesions were very firm."</p>
16	Dr. Merriman, Lancet, 1844, vol. ii, p. 298	Child	Not given	4 days, death	<p>Cæcum, appendix, and ileo-cæcal valve invaginated into colon</p> <p>Probably might have been felt if examination per anum had been made during a straining fit.</p>

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
17	Mr. Snow, <i>ibid.</i>	Child	Not given	Death	Ileum into sigmoid flexure	Same as No. 16.
18	Markwick, <i>Lancet</i> , 1846, vol. ii	4 mos.	Blood passed; the symptoms began soon after birth	4 mos.? death	The colon was intussuscepted into the sigmoid flexure; it was impossible to withdraw it	Ditto.
19	Clark, <i>Lancet</i> , 1849, vol. ii, p. 206	6 mos.	Left side of abdomen hard and prominent	2 days, death	Ileum, cæcum, and colon intussuscepted into descending colon	Ditto.
20	Burford, <i>Lancet</i> , Oct. 31, 1840	6 mos.	Passage of blood	3 days, death	<i>Post-mortem.</i> —"Was surprised to find the rectum fall." Ileum, cæcum, and colon invaginated into sigmoid flexure and rectum.	
21	M. Robin. <i>M.</i> Hévin's <i>Mémoire</i> in <i>Mém. de l'Acad. Roy. de Chir.</i> , t. xi, p. 324	3½ years	Prolapsus after suffering more or less for three months	6 days, death	Cæcum and greater part of colon in the rectum. "It was found impossible to withdraw the invaginated intestine, it had contracted strong adhesions."	
22	M. le Blanc. <i>M.</i> Sabatier's <i>Mémoire</i> (<i>Mém. de l'Acad. Roy. de Chir.</i> , t. xv, p. 35)	A child	Prolapse six or seven inches in length	15 days, death	Ileum, cæcum, and colon, were invaginated into the rectum; it was impossible to reduce this.	
23	M. Puy, quoted by M. Sabatier (<i>l. c.</i>)	40 years, M.	Prolapse to the extent of about six inches; the first attack occurred about two months before death, but he apparently recovered from this	16 hours, death	Ileum and colon invaginated into the rectum.	

24	—	—	Death	The sigmoid flexure of the colon was invaginated into the rectum to the extent of 13 inches.
	MM. Roux et Lavernet, quoted Dict. des Sciences Med., vol. xxiii, p. 560			
25	9 mos., M.	Discharge of blood; tumour felt in the left iliac fossa	60 hours, death	Ileum, cæcum, and colon invaginated into colon and rectum.
26	4 mos., M.	Slime and blood discharged	68 hours, death	Ileum and colon into colon and rectum.
27	11 mos.	Hæmorrhage	62 hours, death	Ileum, cæcum, and colon into colon, close to sigmoid flexure ? Could have been felt.
28	16 mos.	Hæmorrhage, and large tumour felt in left iliac region	48 hours, death	Ileum, cæcum, and colon into sigmoid flexure.
29	9 mos.	Hæmorrhage, and tumour felt in left iliac region	40 hours, death	Ileum, cæcum, and colon in sigmoid flexure and rectum.
30	M.	"The valve of the colon at last got as low as the anus, and when he went to stool he only emptied the ileum"	Death	Ileum, cæcum, and colon in sigmoid flexure and rectum.

MM. Roux et Lavernet, quoted Dict. des Sciences Med., vol. xxiii, p. 560

Dr. Ash, quoted by Hunter, Trans. Soc. for Imp. Med. Chir. Knowledge, vol. i, p. 108

Mouro, sen., Morbid Anat. Alimentary Canal; Dr. Smith, An. Journ. Med. Sci., Jan., 1862

Mr. Clarke, Lancet, Feb. 10, 1838 (Gorham, Guy's Hosp. Rep., Oct., 1838, p. 331)

Dr. Baer, Am. Jour. Med. Sci. (Gorham, l. c.)

Mr. Cunningham, Med. Gaz. Sept. 15, 1838 (Gorham, l. c.)

Mr. Whately, Phil. Trans., vol. lxxvi, p. 305

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
31	Dr. O. Ferral, Dub. Path. Soc., Lond. Med. Times, Jan. 16, 1847 (Dr. Smith, l. c.)	12 mos., M.	Intense pain, passage of mucus and blood	6 days, death	Ileum and cæcum into colon; there were two orifices, one leading into the ileo-cæcal valve, the other into the appendix	Probably could have been felt.
32	W. S. Partridge, Prov. Med. and Surg. Journ., May 3, 1848 (Dr. Smith, l. c.)	4 years	Passage of blood and mucus	3 days, death	Ileum, cæcum, and colon invaginated into rectum	Ditto.
33	Dr. Harland, Med. and Phys. Res., Philad., p. 565 (Dr. Smith, l. c.)	5 mos., F.	Prolapse	Not stated, death	Ileum, cæcum, and colon into colon and rectum; the cæcum protruded from the anus.	Probably could have been felt.
34	Mr. Davies, Med. Repos., Dec., 1824 (Dr. Smith, l. c.)	6 years, F.	Diarrhœa, passage of mucus and blood	8 months, death	Ileum, cæcum, and colon into colon and rectum	Ditto.
35	Dr. Kennedy, Dub. Journ. Med. Science, March 4, 1844 (Dr. Smith, l. c.)	4 mos.	Passage of blood	1 day, death	Ileum, cæcum, and colon were invaginated	Ditto.
36	Mr. Perrin, Lancet, March 26, 1853 (Dr. Smith, l. c.)	3 mos.	Bloody stools	2 days, death	Ileum, cæcum, and colon. in descending colon and sigmoid flexure	Ditto.
37	Dr. Smith (l. c., Case 34)	3 mos., M.	Passage of mucus and blood; prolapse twelve hours before death	7 days, death	Ileum, cæcum, and colon into colon, &c.	

39	Journ. Med. Sci., October, 1827 (Dr. Smith, l. c.)	—	Tumour felt two or three inches from anus	evacuation; prolapse after seven or eight days, and the next day 23 inches separated	recovery	An invagination was found in the rectum.	sloughing.
40	Jacobi (Dr. Smith, l. c.)	18 mos., M.	Prolapse; after reduction a tumour could be detected with an opening at the lower part like an os tinæ; large enematæ were given, and a long probe of whale-bone armed with sponge was used, but without success	Death	A few days, death	The invagination began just below the upper part of the sigmoid flexure.	
41	Mr. Stanley, Lancet, March 11, 1826, p. 813	Middle age, F.	Prolapsus	Death	6 days, death	A surgeon gently pulled at the intestine, and a yard and three inches came away; it proved to be a portion of ileum.	
42	Mr. Howship, Ed. Med. & Surg. Journ., April, 1812	4 mos., F.	Passage of blood; injections failed	5 days, recovery	14 days, recovery	The lower part of the colon and the upper part of the rectum were invaginated into the rectum. He regrets "that this was not felt during life."	? Could probably have been felt. Recovery by gangrene.
43	Mr. J. W. Bowman, ib., Oct. 1813	11 years, F.	Obstruction of the bowels and every symptom of approaching dissolution	9 weeks, death	—	Portion of colon, cæcum, and mesentery, measuring 19½ inches, passed by stool	Ditto, ditto.
44	Mr. Valetutine, ib., April, 1826	40 years, M.	Passed 23 inches of colon	Passage of mucus and blood; improvement for three weeks; at the end of forty-six days, prolapse; this increased till as much as 6 inches protruded	9 weeks, death	The lower part of the ileum had passed along the whole length of the colon and out at the anus; it was still pervious; there were evidences of progressive sloughing.	
45	Mr. Sydney Jones, Path. Tr., vol. viii, p. 179	4 mos., M.					

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
46	Mr. Holmes, Path. Trans., vol. viii, p. 177	40 years, M.	Tumour felt at a distance of about half an inch from the anus	10 days? death	The sigmoid flexure of the colon and the upper part of the rectum were invaginated. In the middle of the ascending colon was a ragged opening, from which faeces had escaped.	
47	Mr. Ballard, ib., p. 185	6 mos., F.	Blood-stained fluid passed per anum	4 days, death	Ileum, cæcum, and colon invaginated into rectum within two inches of the anus.	
48	Dr. Quain, ib., vol. x, p. 160	5 years, M.	Pain in region of bladder; no passage of blood; sickness and constipation	4 mos., recovery	8 inches of the ileum, the cæcum, and 4 inches of the colon were passed by the anus	? Could probably have been felt. Recovery by sloughing.
49	Dr. Buchanan, ib., p. 171	7 mos., F.	Passage of blood; physical examination of the abdomen negative	53 hours, death	Ileum, cæcum, and colon invaginated into sigmoid flexure and rectum to within half an inch of the anus. The intestine could be withdrawn without special difficulty	Stress is laid on the importance of an anal examination.
50	Mr. Nunneley, ib., vol. xi, p. 109	3 years, M.	Passage of a little bloody mucus	11 days, death	A foot of ileum had passed through the ileo-cæcal valve into the large intestine; there was no appearance of inflammation whatever, neither lymph nor blood was effused	? Could have been felt.

TRANS., VOL. IXXVI, p. 305	F.	CHILD NOT SUCH FOR THREE WEEKS	URGENT	round substance in the rectum, with an opening in the middle, not unlike an os-tinæ; the finger passed completely round this between it and the wall of the rectum; the enclosed intestine was in a state of commencing gangrene, but could be easily withdrawn; a portion of ileum contained was uninvolved, so was the appendix cæci.	Recovery after reduction by a bougie.
52	Dr. Osborne, Aitken's Medicine, vol. ii, p. 814	A child	Tumour felt in the rectum; at the end of thirty-four hours it almost presented at the anus; an elastic bougie was passed into the orifice and pushed up; it carried the intestine with it, but "more owing to straightening of the canal than any force used"	2 days? recovery	—
53	Dr. Thomson, Edin. Med. Journ., pp. 300 and 316. From an Italian source	40 years, F.	After seven or eight days colic symptoms; passage of part of colon, the cæcum and its appendix; a month later there were still colicky symptoms, and a hard, circumscribed tumour could be felt in the left iliac region	50 days, death	When the omentum was raised, two openings were found in the colon, one of which received the ileum and its mesentery; the omentum performed the part of an outer coat, so that no faeces escaped from the intestine. The rectum "appeared full of faeces, but on being cut up the ileum and mesentery, for a Parisian foot, were found pushed into the colon as far as the rectum."

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
54	Dr. Greig, Edin. Med. Journ., Oct., 1862, p. 312	4 mos., M.	Passage of blood; tumour in left side of abdomen; an euema could not be thrown up	42 hours, death	Ileum, cæcum, and colon invaginated into rectum to within an inch of the anus. "After removal of the tumour the cæcum was easily drawn out of the colon, and restored to its natural position, but the greatest difficulty was experienced in getting the swollen, small intestine reduced through the ileo-cæcal valve, which seemed even then to be in a spasmodic condition"	Dr. Greig narrates four other cases which recovered under the use of inflation, but in these the tumour was on the right side, probably short intussusceptions. In one of them he had to inflate several times.
55	Dr. Greene, Brit. Med. Journ., March 18, 1871, p. 278	4½ mos.	A tumour, much resembling a small sausage in shape and density, was felt on left side of abdomen; passage of slime	Several days, death	Ileum, cæcum, and colon into descending colon and sigmoid flexure.	The distance from the anus is not stated, but possibly by pressing on the abdomen, and examining during straining, the finger might have reached the "upper part of the rectum."
56	Dr. J. St. C. Gray	5 years, M.	Tenesmus, passage of blood at first, then of blood-stained mucus. "There was considerable tympanites, but nothing was ascertained tending to throw light on the case, either by percussion or by examination of the rectum by the finger." Turpentine euemata, then various remedies	8 days, death	There was no trace of peritonitis; colon was invaginated into colon and upper part of rectum; the cæcum was not involved	Recovery after gangrene.
57	Mr. King, Lancet, June 17, 1854	6 years, M.	Suddenly seized with symptoms of ileus; in four days convulsions and insensibility	11 days, recovery	The cæcum and appendix, with part of the ascending colon, passed per anum; afterwards the right half, anteriorly becoming swollen	

ZUG. II. P. 4, 215, 1858. Schmidt's Jahrb., 101, 1859, p. 321	TITRE DE LA MALADIE, CONSIDÉRÉE EN GÉNÉRAL	TITRE DE LA MALADIE, CONSIDÉRÉE EN PARTICULIER	TITRE DE LA MALADIE, CONSIDÉRÉE EN PARTICULIER
59 M. Sobaux. (M. Hévin, Mém. de l'Acad. Roy. de Chir., t. xi, 1784)	Adult, M.	A portion of colon, 23 inches in length, passed per anum after about three weeks' illness	Ditto.
60 M. Fauchon. (M. Hévin, l. c.)	48 years, M.	The whole of the cæcum, with 6 inches of the colon, and the same length of ileum, were passed after twenty-five days' illness; the patient seemed well afterwards, but died three days later	Death after gangrene and separation of gut.
61 Dr. Thomson, Edin. Med. Journ., 1835, p. 301	40 years, M.	The man was thrown down and trampled on; after some weeks he suddenly felt something in the rectum; at last protrusion occurred, and when he laid hold of the mass extruded he pulled away intestine	Recovery by gangrene.
62 Dr. Thomson, l. c., p. 304	11 years, F.	Very severe abdominal symptoms; threatened dissolution	Ditto.

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
63	Dr. Thomson, l. c., p. 305	24 years, M.	Around the umbilicus was an oval swelling larger and longer than a turkey's egg	40 days, recovery	The whole of the cæcum with its appendix was discharged per anum	Recovery by gangrene.
64	Dr. Thomson, l. c., p. 308; also 1836, p. 374	4½ years, M.	Colicky pains in the belly, with passage of blood; protrusion of intestine from anus five days before its separation	Some days, recovery	The boy, while suffering from smallpox, passed the whole of the cæcum with its appendix, and part of the ileum	Ditto.
65	Dr. Baillie, Trans. Soc. Improvement of Med. Knowledge, vol. ii, p. 144. (Dr. Thomson, l. c., p. 312)	50 years, F.	Frequent passage of blood and vomiting; diarrhœa came on and lasted for many days	"Many days," death	No autopsy. A yard of colon was passed three weeks before death	Death, although the gangrenous bowel had passed.
66	Dr. Thomson, l. c., p. 313	30 years, F.	Colic, fever, stercoraceous vomiting. On the 8th day the cæcum and a part of the colon were passed	Death	She died a fortnight after the separation of the bowel in connection with a confinement of a stillborn child; the whole abdomen was filled with purulent serum	Ditto.
67	Dr. Thomson, l. c., 1836, p. 378	40 years, F.	After various abdominal symptoms, passage of a membranous substance	Recovery	It was considered to be the cæcum which was passed	Recovery by gangrene.
68	Dr. Thomson, l. c., p. 378	35 years, M.	Dysenteric symptoms; about 18 inches of colon passed	15 days, recovery	—	Ditto.
69	Dr. Thomson, l. c., p. 380. From Meckel	17 years, F.	"A fever, attended at first with constipation, and then with diarrhœa"	4 weeks, death	Separation of cæcum and appendix, and later, of whole of transverse and ascending colon with portion of ileum, thirteen	Death long after gangrenous detachment of the bowel.

70	Dr. Thomson, l. c. p. 380	7 years, F.	Fever and pains in the abdomen; in the region of the loins was felt a pretty hard swelling of the size of a goose's egg; at length the whole cæcum with its vermiform appendix was discharged per anum	Death	At first the patient improved after passage of the intestine, but was at last carried off by fever, colic, vomiting, and diarrhoea. The cæcum and part of the ileum had evidently been invaginated into the colon, and had sloughed off.	The cause of death is not stated. Death occurred after gangrenous separation.
71	Hill, Month. Jour. Med. Sci., vol. v, 1845, p. 572. (Dr. Peacock, Path. Tr., xv, p. 122)	65 years, F.	Constipation, vomiting, diarrhoea; constipation for a week; then in five days passage of <i>forty-four</i> inches of intestine; forty days later she sank, exhausted	53 days, death	The portion passed proved to have been the sigmoid flexure; only fourteen inches of colon remained and terminated in a cavity containing faeces from which the rectum arose.	
72	Dr. Peacock, l. c. p. 122	67 years, F.	Pain followed by diarrhoea; a large portion of the rectum and colon was passed on the fifteenth day	15 days, recovery	She ultimately recovered, but suffered from pain in the abdomen.	
73	Dr. Hunter (Jedburgh), Lancet, Mar. 9, 1872	9 mos.	Was not very fretful nor peevish; took the breast well till near the end; was very thirsty, and drank eagerly of water; vomiting was almost constant, though it was not faecal at any time. For the last fortnight the ileo-cæcal valve protruded at the anus often to more than an inch beyond, and at other times lay up in the rectum when put up	20 days, death	The invagination had begun at the ileo-cæcal valve.	

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
74	Wilson, Clinical Record, Feb., 1870. (Virchow's Jahreshb., 1870, Bd. ii, Abth. 3)	4 mos.	—	7 days, death	Ileum, cæcum, and colon invaginated into the colon and upper part of the rectum.	
75	Wagner, Jahrb. f. Kinderhik., n. f. iii, 343	2 years, M.	The tumour was felt on rectal examination; after inflation the tumour at once disappeared	Recovery	The child remained well at the end of eighteen months	Recovery after inflation.
76	Ditto	4 years, M.	Tumour on the left side of the abdomen; none felt on examination per anum	Recovery	Clysters had no effect. After eight pumpings with bellows a loud report was heard; the next day there were signs of the tumour returning, but the whole disappeared after inflation. The child remained well	Ditto.
77	Dr. S. Wilks, Lancet, May 21, 1870	6 mos., M.	Sickness; blood by rectum; lump felt to left, above umbilicus, which hardened on pressure. On passing the finger into the rectum a round projection could be felt four inches up with a circular orifice in the centre; inflation was used and the tumour disappeared	24 hours, recovery	"Ileum into cæcum probably." The child remained well for about a fortnight, was then brought with a lump again to be felt and had passed blood. The mother declined further treatment, and the case was lost sight of	The result remained doubtful.
78	Dr. Hilton Fagge, Guy's Hosp. Rep., 1869, p. 289	5 years, M.	Pain and abdominal tumour the only symptoms for two (? four) months; symptoms of strangulation with hæmorrhage four days before death. Peculiar	4 months, death	The ileum, cæcum, and colon intodescending and sigmoid colon. Shreds of lymph (adhesions) of no very recent formation united the parts together; no ulceration nor gangrene; the finger could	

79	Ditto, p. 302	1 year	up Had passed blood; twelve inches of intestine (ileum, cæcum, and colon) sloughed	Recovery	—	Recovery by sloughing.
80	K. v. Mosengeil, Arch. f. Klin. Chir., xii, p. 75	7 mos., M.	Doubtful symptoms for fourteen days; then for four days constipation, straining, and propulsion of a tumour nearly to the anus; next day prolapsus; this increased on following day, and the whole was replaced by a catheter. The child appeared well. In a few hours the protrusion returned and no efforts were successful in reducing it. Three days later some mucous membrane sloughed; the next day an artificial anus was formed in the left groin, and an intussuscepted portion of gut found in the portion opened. A catheter could be passed a long way up by the side of the invaginated portion. On the third day afterwards the child died	27 days, death	At first, injections of water and the use of a sound did not succeed, then the latter replaced the intussusception; but later in the same day nothing was of any avail. No post-mortem. The operation performed was of no avail. The operator recommends that in future the incision should be made on the right side. He would have done so here afterwards, but the child was too exhausted	The question of performing abdominal section and replacing the intestine does not seem to have been entertained. It would appear to have been a favorable case. The case is exceptional in respect to the long survival of a very young child.
81	Dr. Steffan. (Dr. Pilz, Jahrb. f. Kinderheilk., n. f., iii, p. 6, 1870)	3 mos., M.	Well-marked symptoms	4 days, death	<i>Post-mortem.</i> —Ileum, cæcum, and colon into descending colon; the folds were covered with blood-stained mucus and what seemed to be layers of fibrine	Cited on account of condition at post-mortem.

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
82	Dr. Ecker (ibid.)	6 years, M.	Well-marked symptoms; passage of portion of small intestine; yet, on the fourteenth day, sudden symptoms of peritonitis	Death	No post-mortem	Cited as fatal result after sphacelus.
83	Dr. O. Groos. Berlin. Klin. Woch., 1870, p. 395. (Central. Jahrb. f. Päd., 1871, Bd. ii, p. 58.)	6 mos., M.	After twenty-four hour tumour in left abdomen and in rectum; third day prolapsus; attempts with finger and with clysters without success; repeated second day	5 days, death	<i>Post-mortem</i> .—Peritonitis. Old adhesions of parts of ileo-colic intussusception to each other. The author remarks that the intussusception must have been of old standing, and that the symptoms came on when the passage previously existing was blocked by the subsequent invagination of small intestine into it.	
84	Dr. Max. Herz (l. c. 1872, Bd. i, p. 5)	7 mos., F.	Sickness and constipation; clysters thrown up on third and fourth days; on fifth day blood and slime passed; no tumour; injection of air; sixth day tumour left side of abdomen and in rectum; on the seventh day convulsions	6 days, death	No post-mortem. See also case of abdominal section.	
85	Dr. Faber, Wurtemb. Corresp. Blatt, No. 25 (Virchow's Jahresh., 1870, Bd. 22)	11 years, M.	Tumour in abdomen just under navel; passage of blood. The tumour was long and tolerably hard	Recovery	After four injections of cold water at different times the tumour disappeared	? Examined by rectum.

	Jahrb., Bd. lvi. 1848, p. 59 (No. 42, Pilz*) (Jahrb. f. Kindhik., u. f., Bd. iii, 1870)	M.		recovery	marked are given by Pilz as cases of intussusception into or beyond the rectum, in which recovery resulted, and for that reason we quote them. He gives no details. We have obtained particulars of the others; they amount to eight recoveries out of forty of such cases. All are given here.]	Schmidt's Jahrb. seems doubtful.
87	Gelman, Jahrb. f. Kindhik., Bd. v, p. 175	8 mos., M.	Seen on fourth day; injections of water of no use; fifth day, air no use; intussusception descending; sixth day, tumour felt in the rectum; after seven injections of water the tumours disappeared	5 days, recovery	On the fifth day calomel was given in two-grain doses every half hour, <i>i.e.</i> ten grains altogether. The author remarks that his case was interesting, because "in spite of the five days' duration, no adhesion of the surfaces had occurred"	In this case perseverance with water injections succeeded after insufflation had failed.
88	Nissen, Ficke und Opp. Zeitschr., Bd. xix, p. 162 (78 Pilz)*	9 mos., F.	Tumour on left side of abdomen; prolapsus; reposition with a sound and injections of water	9 days, recovery		
89	Neumann, Inaug. Diss. Halle, 1842 (79 Pilz)*	9 mos., M.	Colon into descending colon; tumour felt by the rectum; sound used	3 days, recovery		
90	Legoupil, Gerson Magazin. (120 Pilz)*	4½ years, M.	Ileo-colic; prolapse; blood passed	30 days, recovery		
91	Prestat, Journ. f. Kinderkrank., 1863, Bd. xli, p. 310	9 years, F.	Symptoms of intussusception, then prolapse, which was re-placed, but the child soon afterwards passed a mass of intestine	7 days, recovery	Sphacelus on eighth day; the lower end of the small intestine with a fold of mesentery. The child was under observation for a year afterwards.	

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
92	Gieffers, Caspar's Woch., 1815 (156 Pilz)*	13 years	Prolapsus	Recovery	Sphæculus of twelve inches and a half of large intestine	Recovery by gangrene.
93	Van Nes, Schmidt's Jahrb., 1848, Bd. lvi, p. 59	9 mos. M.	Tumour felt in rectum and in abdomen. "After three days symptoms of peritonitis"	3 to 4 days, death	Transverse colon into descending colon and rectum. After death he opened the abdomen on the left side, in front, and then the intestine. He found a long invagination and could not reduce it; he also found evidences of peritonitis.	
94	Dr. H. A. Beach, Boston Med. and Surg. Journ., Nov. 5, 1868	24 years, M.	Tumour felt in rectum after injections had been given and the lower part of the rectum cleaned out	85 hours, recovery	He placed the man "on his shoulders and knees" and gave injections. The intussusception slipped up, but returned when the man moved about four hours afterwards. It was again driven up Invagination of ileum, cæcum, and colon into colon.	Recovery by injective treatment.
95	W. Pepper, Phil. Med. Times, Sept. 1, 1871 (Virehow's Jahrb., 1871, Bd. ii, p. 152)	6 mos.	—	4 days, death		
96	Cooke, New York Med. Record, May 1, 1871	An adult ?	Constipation; passage of slime for three or four days, then an intussusception detected in the rectum	5 days, recovery	The tumour was pushed up as far as possible, then driven still further by injections while the patient was on knees and elbows. The next day the injection was repeated and an evacuation followed	Recovery by injections.

97	Kjelberg and C. Blix (Virchow's Jahresb., 1871, Bd. ii, p. 606)	11 mos., F.	Tumour felt in abdomen and by rectum; attempts at reduc- tion fruitless	Death	Invagination of ileum and colon into descending and sigmoid colon.	
98	Gactano Moretti, Annali Univ. di Med. (Giugno (Virchow's Jahresb., 1871, Bd. ii, p. 153)	40 years, M.	Symptoms of invagination; prolapse of sixteen inches of intestine; it could easily be pushed back a certain distance, but no farther; finally, it came away altogether	Recovery	The portion of intestine which came away was sixteen inches long, and was believed to belong to the sigmoid flexure of the colon. The patient remained under observation for two months, and was then quite well	Recovery by gan- grene.
99	Dr. Hodges, Bos- ton Med. and Surg. Journ., Aug. 6, 1868, p. 5	3 years	After various symptoms of colic, vomiting came on, and tumour felt in right iliac fossa; none in rectum. Two days later blood passed, and the next day a tumour was found in the rectum. On the following day the child was sent to hos- pital. Tumour felt in rectum, and made visible by the use of a spe- culum. Child in a moribund con- dition. A steel sound was used, and the tumour pushed out of sight, but the child died eight hours later	4 days? death	<i>Post-mortem.</i> —The cæcum was found to present. No statement as to peritonitis	Tumour made visible by use of a speculum.
100	Hachmanu, Zeit. f. gesamt. Med. v. Fricke u. Oppen., Bd. xiv, p. 289 (No. 1 Pilz)	11 weeks, M.	Passage of blood; tumour in abdomen and in rectum	5 days, death	Ileum, cæcum, and colon into colon as far as the rectum.	

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
101	L. Smith, N. York Path. Soc., 1861 (No. 8, Pilz)	3 mos., M.	Passage of blood; tumour in abdomen and in rectum	8 days, death	Ileum through the colon.	See No. 121.
102	Plath, Caspar's Woch., 1839, p. 432 (No. 10, Pilz)	16 weeks, M.	Passage of blood; tumour in abdomen and in rectum	2 days, death	Cæcum and colon into descending colon and rectum; there was no gangrene. The intussusception, it is said, could not be drawn out, owing to constriction at its commencement.	
103	Basedon, Siebold Journ. f. Geburtsk., Bd. vii, p. 512 (No. 26, Pilz)	4 mos.	Passage of blood; tumour in rectum	2 days, death	Colon ascendens in colon descendens.	
104	Dr. Staug, Journ. fur Kinderkrank., 1863, Bd. ii, p. 130	5½ mos., M.	Passage of blood-stained mucus; tumour could be felt in the abdomen, and could be seen through the distended anus; a sound was used and clysters were thrown up without effect	4 days, death	Cæcum and colon into colon (transverse). There was a plastic exudation on the layers of intestine in the intussusception, but it did not amount to much; the layers were cut open.	
105	Schwarzelder, Gaz. Hebdom., 1857, p. 583 (No. 73, Pilz)	7 mos., F.	Blood-stained mucus passed; prolapse; tumour in abdomen noticed	6 days, death	Ileum, cæcum, and colon into colon and rectum; no peritonitis. (Quoted in Gaz. Hebdom. from the Cincinnati Med. Observer, July, 1857, p. 295; case under the care of Mr. Wilson.)	
106	Husch, Caspar's Woch., 1838, p. 647 (No. 81, Pilz)	9 mos., F.	Blood passed; prolapse; the cæcum was outside the anus	30 hours, ?	Ileum, cæcum, and colon into rectum. There were no adhesions nor any exudation.	

108	Med. Times and Gaz., 1861, vol. ii, p. 160	F.	large, elongated tumour felt on the left side of the abdomen	death	de-scending colon; no adhesions, no softening, nor gangrenous appearance of any kind.
109	Mr. Matthias Rowe, Loud. Med. Gaz., vol. xv, October 25, 1834	10 mos.	Passage of blood-stained mucus; tumour felt in rectum	30 hours, death	The ileum, cæcum, and colon in the rectum. A drawing of the parts is given. "It was with difficulty that the colon could be drawn out of the rectum."
110	Angustin, Diss. Inaug. Halle, 1836 (No. 103, Pilz)	2 years, M.	Blood-stained mucus; tumour in abdomen and in rectum	11 days, death	Colon transverse, and descending in the rectum.
111	Abercrombie, Diss. cases of the Stomach, p. 123	2 years and 5 mos., M.	Vomiting, pain, passage of bloody mucus and blood; prolapse on second day; the caput coli protruded	2 days, death	Ileum, cæcum, and colon into colon. There was congestion, no adhesions; the ileum was tolerably healthy.
112	Neumaun, Diss. Inaug. Halle, 1842 (No. 109, Pilz)	3 years, M.	Blood passed; prolapse; tumour in abdomen	5 days, death	Colon descending and part of rectum into rectum.
113	Abercrombie, l. c., p. 124	4 years, M.	"It exactly resembled the previous one, except that it was not so extensive"	5 or 6 days, death	Colon descending and part of rectum into rectum.
114	Neumann, l. c. (No. 130, Pilz)	6 years, M.	Blood passed; no tumour noticed in abdomen, but one was felt in the rectum	6 days, death	Ileum, cæcum, and colon into rectum.
115	E. Mayer, Percussion der Unterleibs, p. 85 (No. 140, Pilz)	8 years, F.	Blood passed; prolapse	11 days, death	Cæcum and colon into rectum.

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
115	Thomaun, Hansen, No. 47 (148, Pflz)	11 years, M.	Prolapse	Some days, death	Ileum, cæcum, and colon into colon and rectum.	
116	Dr. H. C. Rose, Med. Times and Gaz., June 8, 1861, p. 597	5 mos., F.	Vomiting; passage of blood	3 days, death	Transverse into descending colon; there were no adhesions. The invagination was about four inches long.	
117	Abercrombie, l. c., p. 124	6 mos.	Vomiting; passage of bloody mucus; tumour in left side of abdomen; injections could not be made to pass up	3 days, death	Ileum and colon into sigmoid flexure; the included parts were very dark coloured, turgid, and in some places ulcerated.	
118	Bock, Schmidt's Jahrb., 146 Bd., 1870, p. 176	10 years, M.	Symptoms of intussusception, and on 17th day tumour detected on left side of abdomen; then it disappeared, owing, as was supposed, to subcutaneous injection of morphia. On 7th day after tumour reappeared; same treatment adopted. Finally symptoms of peritonitis. As the subcutaneous injections seemed so successful at first, no other treatment was adopted the second time	43 days, death	Ileum, cæcum, and colon invaginated into descending colon; perforation above intussusception had occurred.	
119	Specht, Virchow's Jahrb., 1869, Bd. ii, Abth. I, p. 138	36 years, M.	After two and a half months, blood stools, and tumour felt left of abdomen; then symptoms of peritonitis. It is said that no tumour could be felt in the rectum	6 mos., death	Ileum, cæcum, and colon into colon; perforation of the colon to an extent sufficient to allow the cæcum to protrude through it.	

120	Dr. Rogers, New York Med. Record, May, 1871, p. 115	7 years, M.	Passage of bloody mucus; sickness; tumour in left side of abdomen, which changed position and became harder at times. The 2nd day, diagnosis made, and intussusception of air and kneading tried, but given up; then salt and water tried and continued. At first only four ounces could be thrown up; then, by evening, twelve ounces. On 3rd day sixteen ounces; and on 4th day, though bloody mucus passed, eighteen ounces, and the tumour disappeared. Morphia was given also	3 days, recovery	made on the cadaver had shown that the ileum, artificially protruded through the ilco-cæcal valve, could be pushed back by inflation. The attempt to force fluid through the valve, so as to reduce an invagination of the small intestine higher up, succeeded in two or three experiments on the cadaver without difficulty; in others it was impossible to send the fluid past the valve until a little manipulation removed the obstruction. He discussed and advocated the propriety of opening the peritoneal cavity in extreme cases, and applying taxis directly to the bowel affected	jection treatment.
121	Dr. Plath, Caspar's Wochenschr., 1839, p. 432	14 weeks, M.	Passage of bloody mucus; sickness; fullness on left side of abdomen more distinct on the second day	3 days, death	Ileum, cæcum, and colon into colon and rectum. The cæcum was just above the orifice of the anus; the intestine was on the point of becoming gangrenous; it could not be replaced without laying open the outer sheath. (This case occurred before No. 102, in which an anal examination was made.)	
122	Thomas, Journ. f. Kinderkrank., 1866, Bd. xlv, p. 23	22 weeks, M.	Passage of blood; tumour felt left side of abdomen; sickness and constipation; tumour not felt per rectum; clysters were of no effect	4 days, death	Ileum, cæcum, and colon into colon and sigmoid flexure, close to rectum. On attempting to withdraw the small intestine it tore at one part; there was no peritonitis.	

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
123	Thomas, Journ. f. Kinderkrank., 1866, Bd. xlvii, p. 23	6 mos., M.	Passage of blood; sickness; on third day no tumour; on fourth day tumour felt by rectum; a sound pushed the tumour back	3 days, death	Ileum, cæcum, and colon into colon and rectum; no peritonitis. Considerable force was required to withdraw the intestine, aided by a push from below.	
124	Ditto	23 weeks, M.	Passage of blood; tumour in abdomen second day; nothing felt by rectum; clysters, insufflation, and use of sound without avail	3 days, death	Colon ascending and transverse into descending; suspected to have been partially reduced; no peritonitis; no statement as to possibility of reduction.	
125	Ditto	16 mos., M.	Tumour left side of abdomen and prolapse; the latter was pushed back; the anus remained patent. The next day patient seemed better, but still passed bloody slime, and the anus was still patent. On the following day the anus closed; the peristaltic action was not re-established, probably owing to the long constriction	5 days, death	At the post-mortem the intestine was found to have been replaced	Dr. Schütz (Prag. Vierteljahrs., 1868, Bd. ii, p. 10) insists on the value of the relaxation of the anal sphincter as a sign of intussusception.
126	Judson, Southern Med. and Surg. Journal (Gaz. Med., 1837)	5 mos., M.	Sickness; passage of blood; no note as to tumour in abdomen or in rectum. Death appears to have occurred within twelve hours of the passage of the blood and about twenty-four of the first symptoms	1 day, death	Ileum, cæcum, and colon invaginated into rectum, reaching six inches below the sigmoid flexure. It is noted that there was no peritonitis, nor adhesions, nor any effusion, but that the intestine was gangrenous (no description, only black from congestion?). An attempt to withdraw the invaginated bowel was	

127	Herbst, Rust. Magazin, Bd. xvi, p. 105 (44 Pilz)	20 weeks	Passage of bloody mucus; tumour in abdomen	24 hours, death	Descending colon invaginated into the rectum.	
128	Forke, Untersuch. u. Beobach. über d. Ileus, &c., p. 39 (48 Pilz)	25 weeks, F.	Passage of bloody mucus	36 hours, death	Cæcum invaginated into the rectum.	
129	Wiegand, Hufeland's Journ., Aug., 1830, p. 63 (50 Pilz)	6 mos., M.	Passage of bloody slime	2 days, death	Ileum and cæcum into descending colon and sigmoid flexure	Could have been felt?
130	Krukenberg, Jahrb. d. ambul. Klinik., Bd. ii, p. 38 (97 Pilz)	More than a year	Passage of bloody slime; tumour noticed in abdomen	18 days, death	Ileum, cæcum, and colon into the rectum.	
131	Roth, Wurtzb. Med. Zeit., Heft 3, 2, 1862	28 years, M.	Sudden, severe pain over symphysis; shivering and fever; then brownish, jelly-like motions. On the fifteenth day vomiting and prolapse of the rectum occurred, but was reduced. Afterwards a round, hard, ill-defined tumour was felt in the left iliac fossa, and on examination by the rectum an obstruction was met with produced by an oblong, hardish tumour with a slit-like opening (resembling the os tincæ) at its lower end	3 weeks, recovery	The treatment consisted of the use of long-continued warm baths, frequent injections of lukewarm water, and the administration of castor-oil occasionally during a portion of the time. Daily examinations were made per rectum, but it did not seem that the injections had any effect; finally, the tumour disappeared. It is noted as a case of spontaneous reposition; the attempts to effect reduction by surgical procedures being unavailing	Supposed spontaneous reposition.



ON
TRANSVERSE FRACTURES
OF THE
PATELLA.

BY
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SURGEON TO THE LONDON HOSPITAL.

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For some years fractures of the patella have claimed my especial attention, on account of certain features which seem to me to have been somewhat misunderstood. I have had large opportunities for examining them, and have done my best to follow up my cases until their final results became known. Some errors have, I think, become current owing to the result being recorded at much too early a date, and more especially has this been the case as regards "bony union."

On the other hand we are liable to mistakes if we venture to found opinions on museum specimens, to which no life history is attached. My aim has been to connect the circumstances observed in the early stages of these injuries with the conditions discovered in the patient's after-life, or revealed by post-mortem dissection, and thus to get a complete view of the whole case.

With a view to conciseness I have stated my conclusions in a series of propositions. Some of these only express the

general creed of surgeons, whilst others more or less differ from it.

PROP. I.—For the purposes of clinical discussion fractures of the patella may be conveniently divided into those in which separation between an upper and lower fragment is possible, and those in which it cannot occur.

PROP. II.—The cases in which no separation of a distal and proximal fragment is possible, are much fewer in number than the others, with which they have scarcely any feature in common.

In this group we count cases of vertical fracture (probably very rare); those cases of starred fracture from blows in which no mobility is possible, and in which probably the articular surface of the bone is not involved; and the not infrequent cases in which fragments of greater or less size are broken off from the borders of the bone.

In none of these can any important questions of treatment arise, it being clear that the limb should be kept at rest, and probable that in many instances bony union will result.

PROP. III.—The cases in which separation between the upper and lower fragments is possible, are common, and occur especially in adults who follow laborious occupations.

PROP. IV.—The line of fracture is usually transverse or nearly so, and often near the middle. If one fragment be larger than the other, it will probably be the upper one. Sometimes the lower one is very small. In a few cases the bone is broken into three or more fragments.

PROP. V.—These transverse fractures are not infrequently caused by muscular contraction alone.

PROP. VI.—In the numerous cases in which direct violence is applied to the bone at the moment of fracture, there is almost always muscular contraction simultaneously present, and it is impossible to say which takes the chief share in the result.

The patella probably never breaks transversely except when the knee is more or less bent, and the bone balanced as a lever on the condyles of the femur. Sudden and unexpected flexion, or the necessity to prevent such, is more likely to break it than extreme flexion. The position of the fracture may serve roughly to indicate the degree of flexion existing at the time the fracture happened. If the fracture is very low down, probably the knee was but little bent, and thus the greater part of the bone still resting on the condyles. I have met with not a few cases in which, as far as the patient's knowledge went, no violence whatever was received on the bone at the time of fracture. In a majority, however, there is a history of the knee having been struck against something during the fall.

PROP. VII.—If the bone is broken by direct violence at a time when the muscle is not in action, there will be no material separation of the fragments either at the time or subsequently; and the probability is very great that the line of fracture will not be transverse.

Unless the bone be held firmly as a lever by the muscles at the time it receives the blow, there is no reason why the fracture should cross the bone transversely, and the probability is that it would be starred. The fact that most fractures are transverse is therefore an argument in favour of the current belief that muscular action is a main agent in causing them. The fact that direct violence under such circumstances cannot possibly rupture the strong fibrous structures by which the bone is everywhere surrounded, explains the circumstance that no subsequent separation is likely to occur.

PROP. VIII.—The existence of a wide separation proves that the muscle was in vigorous contraction at the time the bone was broken, and also that the fibrous structures have been extensively lacerated.

The fibrous structures at the sides of the patella are especially strong and are firmly united with it. To admit

of wide separation there must necessarily be a considerable laceration into the front of the joint. Such laceration is possible only at the time of the accident.

PROP. IX.—In all cases of transverse fracture involving the whole thickness of the bone, the knee-joint is necessarily implicated, and it is probable that blood will find its way into the joint, and that synovitis with effusion will follow.

The quantity of blood in the joint is probably often very small, but in severe cases it may be considerable. In one case under my observation it distended the joint, and it was necessary ultimately to evacuate it by incision. The effusion of synovia is an almost constant result, and is, I believe, a very important complication of the case.

PROP. X.—There is no persistent contraction of the quadriceps extensor, or of any part of it, after transverse fracture with separation. When the spasmodic effort which attended the fracture ceases, the muscle usually remains completely and permanently at rest.

My experience has invariably been to find the front of the thigh soft and flabby, the quadriceps being quite at rest. I have scarcely ever known the muscle pass into spasm during examination, but in reference to this remark I must state that it does not apply to the first few hours after the accident, respecting which my knowledge is very limited.

PROP. XI.—The subject of a transverse fracture of the patella is usually quite unable by volition to put the muscle into action, and there is no reason to believe that the muscle is liable to attacks of accidental spasm.

If you ask a man with a recently broken knee-cap to try to lift his limb from the bed, he will reply helplessly, "I cannot;" if you urge him he will get emphatic and

declare, "Sir, I couldn't do it if you gave me the world," but all the time his quadrieeps will make not the slightest movement.

PROP. XII.—After transverse fracture with separation, the fragments become of course widely parted if the knee be bent. If, however, the knee be kept straight, their relative position will not be in the least altered by lifting the limb from the couch and elevating it at right angles with the trunk.

The degree of separation produced by bending the knee after transverse fracture varies considerably in different cases. In some the upper fragment does not follow downwards at all, owing probably to extensive laceration in the fibrous structures. In others, although the fragments are quite movable laterally, there is scarcely any gaping in flexion. The experiment is not one which it is wise to try often, nor except with the utmost gentleness. It is not justifiable except in the earliest stage.

PROP. XIII.—The bringing down of the upper fragment is not as a rule in the least facilitated by the common practice of elevating the limb.

In an uninjured limb at rest in the straight position the patella can be moved upwards or downwards for about three fourths of an inch, the ligamentum patellæ restricting its movement upwards, and the fascia preventing its further passage downwards, whilst the rectus muscle remains perfectly flaccid and offers no sort of impediment. Exactly the same condition of things is met with after transverse fracture. I have tried the experiment of lifting the limb dozens of times, and with great care, but never found the upper fragment to alter in position in the least. Care must of course be taken to make the movements gently, and not to let the knee bend. For years I have never treated fractures of the patella except in the longitudinal position.

PROP. XIV.—The causes of difficulty in bringing the upper fragment down are several.

1st.—Effusion into the joint lifting the fragments apart.

2nd. Swelling of the torn soft tissues.

3rd. Contraction of the fibrous structures (chiefly met with when the fragments have been pushed apart by swelling which has lasted for some considerable time).

4th. The difficulty there is in getting well hold of the upper fragment, especially when much swelling is present.

Of these probably by far the most constant and the most efficient cause is effusion into the joint. If you examine a case in which there is little or no effusion, there will be little or no difficulty in bringing the fragments close together. In such cases also it is probable that the laceration is but slight, for the laceration, and the effusion which results, will usually be in relation.

The effect of distension of the joint from within in lifting apart the fragments can easily be imagined, and no doubt effusion into the cellular tissue, between the layers of fascia, &c., will do something towards making the parts rigid, and thus in preventing approximation.

In proportion to the external swelling is the difficulty of getting external pressure, whether manual or by pads, to act efficiently on the upper fragment. The influence of pressure on the sides of the distended joint in making the fluid bulge upwards into the laceration must never be forgotten.

PROP. XV.—In the majority of cases it is easy to force the fragments close and thus obtain crepitus.

PROP. XVI.—The difficulty of securing them in apposition will be exactly in proportion to the amount and duration of the swelling.

I have found much less difficulty in pressing the fragments together, so as to produce crepitus, than books would

lead us to expect. If difficulty has occurred, it has been from the swelling.

PROP. XVII.—The muscle scarcely ever offers any impediment to the approximation of the fragments.

I do not recollect a single case in which, after the first day or two, even forcible pressure on the upper fragment excited any degree of muscular contraction.

PROP. XVIII.—No proof has yet been afforded that transverse fractures of the patella with considerable separation at the time ever unite by bone.

The only proof which we ought to accept on this point is the section of a specimen concerning which the history is definite. There may be such, but I do not know where to refer to them.

PROP. XIX.—It is quite impossible on the living subject to ascertain whether or not bony union has occurred.

In many of my cases at the time of the patient's discharge from the hospital, the fragments were so firmly bound together that I could not produce the slightest lateral movement. In one or two of such cases I have found, by the subsequent stretching of the uniting medium, that it certainly was not bone. A specimen in the London Hospital museum shows such close union, that without section it would have been impossible to say that it was not bone.

PROP. XX.—Transverse fractures with separation may have several different events.

1st. and best.—Close and permanent union of the fragments by fibrous tissue, so that no movement whatever can be detected, and so that section of the bone is needed to show that it is not true bony union.

2nd.—Union by a short and strong ligament.

3rd.—Union by a long ligamentous band which may stretch indefinitely.

4th.—Entire absence of any uniting medium between the bones; the fibrous capsule of the joint, bursa, &c., being the only means of connection.

After six weeks' rest and ordinarily careful treatment by back-splint and strapping, it is, I think, easy to obtain close and firm union, in a fourth or even a third of the cases. In another third the union will be close and strong, but still, on forcible pressure, permitting of slight lateral movement, whilst in the remainder union will be weak and with some persisting separation. In the worst cases the separation ought not at this period to be more than a quarter of an inch.

All statements of results made at the date of the patient's leaving the hospital are utterly untrustworthy, for in some of the best cases the uniting medium will afterwards stretch. The real result is not known until the patient has been allowed to walk about and bend his knee. A year's interval, at least, ought to be allowed, and even after this it is possible that the fragments may become much more distant.

The assertion that non-union, or membranous union, is a frequent result is, I think, a fallacy.

In all the cases that I have examined at the expiration of treatment, there has been reason to believe that the fragments were connected by a fairly thick uniting medium. Thus they could not be separated beyond a certain length, nor could much lateral movement be produced.

The specimens so common in our museums of wide separation, and in which the thickened capsule, bursa, &c., are the only connecting structures, are, I suspect, from cases in which premature flexion had been permitted, and the new material stretched until practically lost.

PROP. XXI.—The chance of the best result is greatest in cases in which but little swelling is present, and the fragments are easily approximated; but it by no means always follows that, in cases in which the fragments easily lie quite close, firm union will result.

PROP. XXII.—The causes of weak union, or of non-union, are probably two.

1st. The fact that the fragments are usually at some little distance from each other during the first week or two of treatment.

2nd. That the space between the fragments is occupied by a fluid secretion (synovia).

Synovitis is an almost invariable consequence of transverse fractures of any degree of severity, and its effusion usually lasts ten days or a fortnight. During this period approximation is difficult and in many cases cannot be attempted.

Without attributing any special effect to synovia, as synovia, in preventing union, it can easily be understood that the presence of any fluid between the ends of a fractured bone must tend in a most important manner to prevent the consolidation of new material. It will supply a menstruum in which the lymph-cells may be washed away and the plasma dissolved.

PROP. XXIII.—Amongst the causes which help to make actual bony union so exceedingly infrequent, it is, perhaps, fair to suggest that the patella differs from most other bones in having periosteal fibres on one surface only, and that from the direction in which the fracture-force is always applied, all those fibres must invariably be ruptured.

PROP. XXIV.—The objects to be attended to during the treatment are—

1st. To keep the knee straight.

2nd. To get rid, as quickly as possible, of all effusion from the joint.

3rd. To avoid all pressure on the sides of the joint, since such pressure must inevitably force the synovial fluid to bulge forwards into the laceration, and thus separate the fragments.

4th. To secure the lower fragment as high, and the upper one as low, as possible.

If the effusion be but little, an attempt may at once be made to secure co-aptation; but if otherwise it is better, I think, to wait a while and to use ice to hasten absorption.

PROP. xxv.—Some unexpected results—hitherto, I believe, not described—frequently occur and in some of the best cases prevent the hopes of the surgeon from being fulfilled. Amongst these it may be mentioned that the quadriceps muscle undergoes a very remarkable wasting during the patient's confinement to bed. This wasting is altogether out of proportion to that of the other muscles of his limb and is almost invariably attended by corresponding loss of power.

The wasting of the extensor muscle is, I believe, an almost constant result. If you measure the two thighs, that of the injured side will be much less in girth than the other, and examination will show that the loss of substance is almost wholly in front, the flexors being still plump and large. The patient will be able very easily to put his flexors in action, whilst he will be almost unable to attempt extension, or will do so in a very feeble manner. In many cases he will be absolutely unable to do so, and attempts to lift the limb will result in his bending the knee a little, and then accomplishing a slight elevation chiefly by means of the sartorius.

It is clear that mere disuse will not account for these results; for the flexors must have been, equally with the extensor, at rest during treatment. The explanation must probably be sought in the same physiological law which disables the patient from calling the muscle into action in the early part of the case.

PROP. xxvi.—In probably more than half of the cases of

transverse fracture of the patella, the quadriceps remains ever afterwards thin and weak, whilst in a not inconsiderable number, it is completely atrophied.

Even in the best cases,—in which the patient regains good power over the extensor and can lift his leg, or kick vigorously,—it is still usual to find the muscle much less bulky than its fellow. In a large proportion the loss of bulk on the front of the thigh is perceptible both to the eye and the hand. I have met with at least six cases of absolute atrophy, so that the bone was quite bare. Most of these were old cases which had been variously treated, but one was in a patient of my own. I do not believe that details of treatment have anything to do with causing or preventing this curious result.

PROP. XXVII.—In almost all cases of fracture with separation, the structures connected with the upper fragment acquire a degree of rigidity during the treatment, which afterwards prevents the upper fragment from easily passing downwards in flexion of the knee.

The condition referred to is, I suppose, one of atrophic contraction. Hunter thought that when separation of the fragments resulted, the extensor muscle must remain too long for its work, and that the patient had to be educated to unusual exertion of it in order to make it act upon the patella and tibia, but, probably, the reverse is the case, and atrophic shortening is the real result.

PROP. XXVIII.—Thus the real danger as to stretching a ligamentous union, is in letting the patient *bend the knee*, there being little or no danger in allowing him to use his quadriceps.

The function of the extensor muscle has been greatly diminished, both as regards ability to contract and also to relax. It can do neither well. It can neither allow the patella to move downwards in flexion, nor draw it much upwards in extension. After a time it will improve in both

respects; but, meanwhile, if flexion have been permitted, the uniting ligament will certainly have been stretched.

PROP. XXIX.—Unless the surgeon has in any case deliberately abandoned all hope of benefit from keeping the ligamentous union short, he ought, on no account, to permit even passive flexion of the knee.

Several authorities, more especially Mr. South (see his edition of Chelius), recommend systematic flexion in the sitting posture at an early period, in order to prevent stiffening of the joint. If my conclusions are correct such exercises are certain to stretch the medium of union, and that often very rapidly. I learned the danger from observing this result.

PROP. XXX.—In any case of weak union the surgeon has his choice,—

1st. To let the patient have motion at the knee with the inevitable result of stretched ligament, or

2nd. To keep him permanently with a straight knee and wearing an apparatus.

It may be doubted whether there is any third possibility.

I apply these propositions to all cases in which the fragments are united by fibrous tissue, but not so firmly but that lateral movement can be produced between them. In all such, if flexion is permitted within six months, the uniting structures will stretch, and I do not know that the liability does not extend much longer.

PROP. XXXI.—In cases of close and firm fibrous union (so-called “bony unions”) it will be wise to try flexion cautiously. If the patella moves downwards easily then probably it will be perfectly safe.

If the limb can be bent without giving pain, and the whole patella carried downwards as one bone, then the

result is equivalent to bony union, and probably no ill results will follow from use.

PROP. XXXII.—After complete atrophy of the quadriceps a person can walk very well, and may follow almost any occupation.

The main risk is that owing to the weakness of the limb another fall may occur, and the other patella be broken.

I have met with patients in this condition who could walk twenty or thirty miles a day, and one who was a van-driver and accustomed, with ease, to clamber up and down to his box without material inconvenience.

PROP. XXXIII.—The main question for the surgeon in deciding as to whether to advise a patient with weak union to bend the knee or to keep it straight and supported permanently by an apparatus, is as to which plan is the most likely to prevent falls.

The convenience in walking will be found almost equal.

My impression is, that the use of external support adds materially to the patient's safety, and often to his comfort also. I have met with patients, however, who preferred a thin bandage or an elastic knee-cap to a stronger support.

PROP. XXXIV.—The frequency with which the quadriceps atrophies after transverse fractures, suggests the desirability of using galvanism from the first to maintain its nutrition. If used, the limb should, during the process, be raised to the utmost, and the patient made to sit up in bed.

Recapitulation.

Thus then, briefly to recapitulate, I have tried to show—

1st. That a very remarkable state of physiological

inactivity in the damaged muscle results from fracture of the patella.

2nd. That the fact of this inactivity destroys the common explanation of the displacement of the upper fragment.

3rd. That this displacement is usually due, not to muscular traction from without, but to fluid pressure from within.

4th. That the real cause of weak union is the bulging of fluid between the fragments.

5th. That atrophic shortening of the quadriceps often follows these injuries, and, whilst it greatly disqualifies the muscle for extension, renders flexion very dangerous to the uniting ligament.

6th. That an exceedingly interesting and curious occasional result is, permanent and complete atrophy of the muscle concerned.

Should the conclusions which I have stated be confirmed by future experience, it is probable that they will also be found applicable to some other forms of fracture and of injury to tendons. Indeed, it is in the possibility that they may illustrate a physiological law of wide bearing that their chief interest consists.

Some Particulars of a Case of marked Bronzing of the Skin. By JONATHAN HUTCHINSON, Surgeon to the Metropolitan Free Hospital.

The following are the brief notes of the illness of the patient whose condition the Fellows will presently have an opportunity of observing :

Emma B—, æt. 34, is a married woman, but childless. She states that on her mother's side there is an hereditary tendency to consumption, and that she has herself always been considered delicate. Fifteen years ago she was for some time an in-patient at Guy's Hospital, under Dr. Bright's care, for what she was told was "disease of the kidneys." From this she recovered, but continued weakly and dyspeptic, until about four years ago, when her health began more decidedly to fall off. One of the earliest symptoms was morning headache; she also got thinner and weaker, and suffered from pain across the lower part of the back. Her friends told her she looked ill, and said she had the jaundice. She has now been for three years continuously under medical care; but states that she does not recollect to have noticed any decided change in colour until about two years ago. That it has been of longer duration is, however, rendered probable by the fact that my friend, Mr. Bletchley, under whose care she was admitted at the Finsbury Dispensary in April, 1856, at once noticed a characteristic degree of bronzing. The change in hue commenced, as far as she is aware, on her hands and face. Coincidentally with its development she has slowly lost flesh and strength. The appetite has usually been very bad, and the bowels inclined to be costive. The catamenia have been regular until quite latterly. The urine has sometimes deposited a lateritious sediment on standing, but more usually has been quite clear. She has suffered greatly from a feeling of general malaise and

debility; but has not been liable to fainting or to any kind of sudden attacks.

At present the skin in all parts is of a deep olive brown. The tint is darkest on certain parts of the face, on the backs of the hands, on the areolæ of the breasts, under the axillæ, and on the abdomen below the umbilicus. The patches of darker colour shade off gradually into the surrounding tint, and there are no abruptly margined patches. On the scalp the colour is very much lighter than on the forehead. On the lips and within the cheeks are numerous small, circumscribed patches of a dead-black colour, as if painted with Indian ink. The body exhales a faint but peculiar and disagreeable odour. The eyes are pearly white where the conjunctiva is visible, and have not the slightest tinge of bile. The urine has been examined, and does not contain bile. The feet and legs are much less deeply coloured than other parts. Occasionally slight œdema of the feet is present. From having been stout in early life she has now become thin and spare. With regard to tastes for different articles of food, no very marked change has been noticed. She never could eat fats or much butter, and dislikes them now rather more than formerly. The treatment to which she was subjected in the former part of her illness consisted in the exhibition of iodides and mild mercurials, together with tonics; but for the last eight months quinine and liberal diet have been the only measures prescribed.

She has been under my own observation for about five months, and during that period I have not noticed much change either as regards the tint of the skin or general health.

In venturing a very confident opinion that the case before us is a genuine example of Addison's disease, I would cite the following as the grounds of that diagnosis: 1. The change in colour is very marked, and has been slowly progressive. 2. The tint was first observed, and is now deepest, on those parts which are exposed to the light. 3. There are patches of black deposit on the mucous membrane of the lips and cheeks. 4. The patient was out of health at the time that change in colour was first noticed, and her debility has increased *pari passu* with its development. 5. Her present cachexia corresponds closely with that observed in other cases of diseased supra-renal capsules.

There is the debility and profound languor, the deficient appetite, the irritability of stomach, &c. 6. The patient gives a distinct history of her having suffered from radiating pain in the back, a symptom observed in similar cases. 7. There is a certain degree of the disagreeable odour so commonly noted in connexion with pigmentary development.

Should this diagnosis be substantiated in the future, the case will have much interest in reference to the question as to how long life may be prolonged in the human subject after destruction of the supra-renal capsules. A degree of bronzing such as is here present is probably never produced *until both those organs have been wholly destroyed for a considerable time*. Numerous clinical facts go to prove that the deposit of pigment in the skin is consequent on the arrest of their function, which supervenes only very slowly. Yet here we have a patient alive, and likely still to live for some months, more than two years after the change in colour was first noticed. Nor is this fact at all incongruous with other observations, for in several other cases the prolongation of life after the commencement of bronzing has been at least as great. It is evident, then, that these organs can be termed "vital" ones only in a very modified sense, since life may probably be protracted after their destruction to at least a thirtieth part of its average duration. This fact should be borne in mind in attempting to deduce conclusions from experiments on the lower animals.

In conclusion, it may not, perhaps, be uninteresting to the Society to advert to the fact, hitherto, I believe, not mentioned, that in its 'Transactions' is the record of a case of "Bronzed Skin," which occupied the attention of one of its meetings long prior to Dr. Addison's discovery. At page 212 of the thirteenth volume is a narrative entitled "A remarkable case of Disease of the Heart, attended with partial Discoloration of the Skin." It was read in July, 1825, by the late Dr. James Johnson. The subject of the case was a lady who died in her thirtieth year, having for long been ailing, and having, for upwards of eighteen months, presented a peculiar change in the colour of the skin. The tint, which was limited to those parts exposed to the air and light, viz., the face, neck, and hands, is described as having been

as deep as that of a very dark mulatto. "Her muscular strength," adds the writer, "was exceedingly impaired, and she felt an indescribable malaise and restlessness, for which she could assign no reason. She was easily put out of breath by any corporeal exertion, though she could expand her chest and take in a very large volume of air without the least difficulty." We have here a good description of the prominent features in the cachexia of diseased capsules. At the autopsy every organ in the body was found "in a state of the most perfect integrity, except the heart." As to the heart, however, all that is described is, that its right ventricle was thin and distended, its left closely contracted, and having very thick parietes. Now as there was no disease whatever of the valves, and as the heart does not appear to have been enlarged, we may, I think, assume that the condition described was nothing more than that state of death-contraction in former years so often mistaken for "concentric hypertrophy." As there is further no note of the supra-renal capsules having been inspected, the Society will, perhaps, not accuse me of discourtesy if I venture to hint that they were probably not looked at, and express a suspicion that they were the organs really at fault.*

* I am indebted to my friend Dr. Wilks for drawing my attention to this interesting case.

ON

PARALYSIS OF THE INTERNAL MUSCLES OF THE EYE

(OPHTHALMOPLÉGIA INTERNA).

A GROUP OF SYMPTOMS WHICH PROBABLY INDICATES
DISEASE OF THE LENTICULAR GANGLION.

BY

JONATHAN HUTCHINSON, F.R.C.S.,

SENIOR SURGEON TO THE LONDON HOSPITAL; SURGEON TO THE MOORFIELDS
OPHTHALMIC HOSPITAL; AND TO THE HOSPITAL FOR SKIN DISEASES.

Read April 9th, 1878.

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I AM not aware that any observer has as yet associated any group of symptoms with disease of the lenticular ganglion. Nor am I in a position to prove by an appeal to dissections that any group of symptoms can beyond doubt be so associated. The object of the present paper is, however, to point out that a certain combination of signs would be likely to follow destructive disease of that body; and secondly, to show that we do meet with cases in which precisely this combination is present in the most definite and remarkable manner. All the patients in whom I have seen it are as yet living, and I have therefore had no opportunity for post-mortem dissection.

The lenticular ganglion receives branches from three sources, two of them motor, and one sensory; the third nerve, the vaso-motor, and the fifth. The distribution of the sensory filaments proceeding from the ganglion cannot easily be identified in bedside observation, since the front of the eyeball receives nerve-supply from the other branches of the fifth, and it may be conjectured that no appreciable anæsthesia would result from destruction of the small filaments which pass into the ganglion. The motor roots, however, supply important structures, the iris and ciliary muscle, paralysis of which can easily be appreciated.

The symptoms which *à priori* would be expected to result from destruction of the lenticular ganglion are iridoplegia or paralysis of the iris, both as to its circular and radiating fibres, and cyclo-plegia or paralysis of the ciliary muscle. My suggestion is that when this triad of symptoms is present—inability to use the ciliary muscle, paralysis of the dilating fibres of the iris, and paralysis of the contracting fibres of the iris,—without any other form of orbital paralysis, the seat of disease can be in no other structure than the ganglion itself.¹ The diagnosis depends equally upon the presence of all three, and the absence of all others. If for instance any of the recti muscles are affected also, we are at once carried further back to the trunk of the third or sixth nerve; and if only one or two of the separate symptoms forming the triple group were present, the inference would be ready that the disease might be seated either in front of the ganglion or behind it in the trunk of the nerve involved. The ganglion itself is the only place where the two motor nerves (third and vaso-motor) are in contact, and where both may be affected by disease which does not involve any other parts.

¹ My only hesitation on this point is as to whether in some cases the same symptoms may be due to disease near the nucleus of the third nerve. It is not easy, however, in such cases to see why the vaso-motor should be affected. I have, however, seen the pupil motionless and accommodation lost in some cases in which the disease was believed to be in this position; always, however, there were other complications.

Clearly, then, it is easy to state beforehand what are the symptoms that ought to follow destructive disease of this ganglion, and to see that they could scarcely be caused by disease located anywhere else.

The second part of my task is to show that there are cases in which this precise group of symptoms is met with, and afterwards I shall have to make some suggestions as to the probable nature of the disease in question.

Of the three structures named it is probable that the circular or constrictor fibres of the iris are supplied solely by the third nerve, that its dilator or radiating fibres are innervated solely by the vaso-motor, and that the ciliary muscle although perhaps dependent in some degree upon both nerves for perfection of function, is chiefly supplied by the third. Any one of these muscular structures may be paralysed alone, and although combinations sometimes occur which it is difficult to interpret,¹ yet it is beyond doubt that the vaso-motor nerve may be wholly paralysed without cycloplegia, whilst the latter is a usual concomitant of dilated pupil due to paralysis of the third nerve.

The first case which drew my attention to this subject occurred in 1865, and was one of the most important that I have seen because the disease was slowly aggressive, and I was able to watch its several stages. The patient was a clerk (Mr. C—), æt. 34, when he first came under care. In the *left* eye the pupil was contracted owing to complete paralysis of the radiating fibres of the iris; he also had difficult accommodation with the same eye. He remained under my observation till 1868; at which date in the *left* eye iridoplegia was complete, and accommodation wholly lost,

¹ Although probably by far the greater part of the motor-nerve supply to the iris and ciliary muscle passes through the ganglion, it is not certain that these structures are wholly dependant on that body. Experiments in dogs make it seem probable that some vaso-motor twigs to the dilating fibres of the iris pass with the long ciliary, and never enter the ganglion. It may be so in man, and, within limits, the anatomical conditions may possibly vary in different individuals.

whilst in the *right* there was complete iridoplegia, but accommodation was still good. There was a remote history of mild syphilis, but although iodide of potassium was given no benefit followed, and the symptoms slowly advanced.

My next case exactly resembled the early stage of the one just recorded, but my notes do not extend over any long period, and I cannot therefore assert that the loss of accommodation ever advanced quite to completeness.¹ The patient, a gentleman, æt. 35 (Mr. E—), came under my observation in April, 1867. In his *left* eye the pupil was absolutely motionless and accommodation much weakened; with a + 15 lens he read No. 1 Jaeger easily. The other eye was perfect. He appeared to be in good health and denied all history of syphilis. He had suffered severely from sciatica. None of the recti muscles were defective. In a bright light the pupil of the paralysed eye was a little larger than that of the other, but in the shade this relation was reversed, the paralysed pupil remaining stationary while the other dilated.

CASE 3.—In April, 1869, a woman (Mrs. T—), æt. 44, who had been under my care for primary and secondary syphilis in 1865, but had in the interval been quite well, came to Moorfields on account of sudden failure of sight in both eyes. Her pupils were of moderate size, and so extremely sluggish that I was in doubt whether it could be said that they acted at all. She assured me that until three weeks before she could see to thread her needle perfectly, and that the loss of ability to do so had been quite sudden. With + 16 glasses she could just manage to read No. 1 Jaeger; her distant vision also was not quite perfect, in consequence perhaps of some small films which were present in the vitreous of each eye.

CASE 4.—*Complete symptoms of disease of lenticular ganglion in left, partial irido-plegia with weak accommodation*

¹ This case has been published in rather more detail in vol. vi of the 'Ophthalmic Hospital Reports,' p. 142.

in right.—Elizth. B—, an unmarried servant girl, who furnished the subject of this important case came under notice in June, 1873. In the *left* eye there was complete paralysis of the iris and ciliary muscle, whilst in the *right* the pupil was very sluggish, but the ciliary muscle was unimpaired. Not the slightest movement of the left iris could be provoked even by exposure to the strongest light. With a + 7 glass she read 1 Jaeger easily. It was probable from her statements that the symptoms had been slowly advancing for six months. She had suffered from much pain across the forehead. I did not ask the direct question as to syphilis, and I failed to detect anything about her in support of such a suspicion. Although in the *right* eye her power of accommodation seemed for the present nearly perfect, she said that the eye soon tired; and it was her impression that it was failing just as the other had done. This patient attended at Moorfields from June 19th to Oct. 2nd, and took throughout iodide of potassium in five grain doses. At the latter date she had so far improved as to be able with the *left* (paralysed) eye to read No. 4 Jaeger, although not easily. The pupil of this eye was still rather larger than that of the other (in a bright light). No further evidence had been obtained as to its possible cause.

CASE 5.—My fifth case is that of a gentleman, æt. 35 (Mr. T—), who discovered that his *right* eye was failing in 1861.

He consulted an eminent ophthalmic surgeon; his symptom being a certain inability to read whilst vision for distant objects remained good. This eye never regained its power of accommodation, and he was obliged ever afterwards to depend wholly upon the left for reading. Some time later the right eye began to diverge, and for troublesome diplopia he came under my care in April, 1869.

I found his *right* eye slightly divergent, its pupil absolutely motionless, and the function of accommodation lost. With a + 12 glass he read No. 1 Jaeger easily with the affected eye, and the sight of this eye for distant test-types was perfect without a glass. It appeared that the superior,

inferior, and internal recti were all in a slight degree weakened. His other eye was not in any way affected, except that it presented a low degree of myopia. He denied any history of syphilis, and I could detect nothing suspicious. His face was covered with acne. He was subject to "rheumatic aching in the limbs." He had suffered from certain other nervous symptoms, thus, he was liable to a nervous cough which sometimes produced vomiting; he often had severe sick headaches; about a year ago he suffered an attack of most violent neuralgia in the face lasting a few days. These were the only items of evidence I could get in reference to his predispositions. It will be seen that the symptoms had lasted in the one eye for eight years, and that the other had not suffered in any degree. It appeared not improbable that disease beginning in the ganglion had travelled backwards to the trunk of the third nerve.

CASE 6.—Mr. H—, a veterinary surgeon, æt. 36, was sent to me on June 2nd, 1875, from a town in the midland counties. In him the condition was symmetrical, and in both eyes paralysis of the iris was complete and accommodation very feeble. He had had syphilis eight years before, and the details of his attack and treatment were supplied to me by the surgeon who attended him. He had no other symptoms of nerve disease excepting constipation, flatulence, and occasional stabbing pains in his limbs. I treated him for several months but without being able to prove any benefit. Sluggishness of pupils and occasional failure of sight had been noticed for a year before Mr. H— came under my care. When I first saw this patient he read 20 Snellen at twenty feet, but could only read large print without glasses. His optic discs were quite healthy, and his sight with low convex glasses was perfect for near objects. My notes do not state the precise degree of failure of accommodation, but at a subsequent visit he saw No. 5 Jaeger with one eye, and No. 8 with the other. His reason for applying to me was that he could not see to read or write efficiently. The eyeloplegia was, however, neither complete nor constant;

thus, at one of his visits to me he managed with great effort to puzzle out a few words of No. 1 Jaeger. This was after some months' use of mercury and iodine, and after he had somewhat improved. His pupils remained throughout absolutely motionless; I tested them in sunlight and used a lens. The stabbing pains mentioned above had been severe enough to suggest the possible commencement of locomotor ataxy, but there were no symptoms conclusively indicative of that malady. He compared the pains to the pricks of a knife, and said that formerly they had occurred chiefly at night and would often keep him awake. His lower extremities were the parts usually affected. He got rid of these pains under iodide treatment, and is now, I believe, quite well.

Hitherto I have not been able to record in any single case that a cure has occurred. In the following instance, however, it seems highly probable that one of the three functions has been regained, since "paresis of accommodation" had been diagnosed four years before the patient came to me, and from this he had almost wholly recovered when I saw him, the iridoplegia, however, remaining complete.

CASE. 7. In which twelve years after syphilis the ciliary ganglion of one eye was affected, and recovery of the function of accommodation with permanent iridoplegia resulted.—This case is one of especial interest because I am able to feel certain as to the patient's antecedents, and also to state the result of treatment. The patient came under my care in May, 1871, and again on Oct. 8th, 1876.

Mr. S— was treated by Mr. H. J. Johnson, in 1860, for chancre followed by eruption, &c. He remained fairly well until 1870, when he was under Mr. Buxton Shillitoe's care for ulcers in the throat and on the tongue. At this time he also became deaf, and consulted Mr. Harvey. In May, 1871, he was brought to me by his medical attendant, Dr. Reilly, of Bow, on account of intractable syphilitic ulcers on the palate and tongue. I saw him only once, and he was subsequently under the care of different surgeons both in England and America for the same malady, always affecting

the same parts. In Oct., 1876, he returned to me having just come home from the Philadelphia Exhibition, and still suffering from sore tongue and lips. He now told me that his *right* eye had suffered about a year after his first visit to me, and that he had been under the care of one of my Moorfields colleagues on account of it. I found his *right* pupil of the same size as the other, but quite motionless whilst the left acted well. He could see almost perfectly in the distance with the *right*, but could not read so easily nor at so short a distance with it as with the other. His near point was eight inches with the *right*, and five inches with the *left*. He told me that when he applied to Mr. H— he could not see to read with the right eye at all though he could see with it in the distance, and that under treatment the sight of this eye had gradually improved. He considered that the eye was now quite well, and was not aware that the pupil was motionless. Mr. H—'s treatment had consisted in a six months' course of five-grain doses of iodide of potassium and the use of Calabar bean. I was shown Mr. H—'s hospital letter upon which "paresis of accommodation" was written as diagnosis; and on it was recorded that after the use of Calabar bean his near point with the *right* was eight inches, and that of the *left* five inches. It will be seen that we have no measure of the degree in which accommodation had failed, but Mr. H—'s expression in the note and the man's memory of his symptoms may be taken as evidence that the function was very considerably reduced.

Looking at the fact that the pupil is now motionless and neither contracted nor dilated, we may take it as highly probable that in the first instance all three functions were lost, and that the accommodation alone has been benefited by treatment. He has had no other symptoms of syphilis of the nervous system.

General summary.

Whatever may finally be thought as to the recognition of the precise part of the nervous system affected, it is apparent

that we have in the cases described in this paper illustrations of a very curious group of symptoms. The cases for the most part very closely resemble each other. In none of them was the patient very seriously ill, and in only two were there indications of disease of other parts of the nervous system. In none did the disease of the nervous system show any tendency to extend whilst the patients were under my observation, a fact, however, which may perhaps have been partly due to their having been all treated by specifics. Of the eight cases, in five both eyes were affected.¹ In three of the eight there was no history of syphilis (two men and one woman), but in none of these can the absence of history be held to prove absence of the reality. In two of these three only one eye suffered, the disease in one of them having been present eight years without showing any tendency to attack the other eye. All my patients were at an age when syphilitic affections of the nervous system are common; the oldest was forty-four, and the youngest twenty-seven when the disease began. In one case I had myself attended the patient for severe syphilis four years before the eye symptoms, and in four others there was the definite history that symptoms of constitutional syphilis had occurred a few years ago. I cannot but regard it as highly probable that in almost all cases the affection is due to syphilis.

We may note that it does not appear to occur amongst the later forms of tertiary disease, but rather during that period in which after disappearance of secondary symptoms there is usually a long period of latency.

One fact I must ask especial attention to; it is that the paralysis of the iris appears to precede the loss of accommodation, and also to resist treatment longer. In four cases it is expressly stated that the iridoplegia was first developed, and in two of them the patient regained to some extent the use of the ciliary muscle. In none was the failure of accommoda-

¹ As regards symmetrical occurrence the facts are very similar to what we find in the case of syphilitic choroiditis, in which frequently both eyes are affected, sometimes only one, and but rarely both in quite equal severity. The two conditions occur also at almost the same periods after the primary disease.

tion the first symptom, a fact the more important when we consider that it causes a condition which the patient is not likely to overlook, whereas a motionless pupil may easily escape attention. It is, I admit, difficult to explain, on the theory that the disease is in the ganglion, why the ciliary muscle and the sphincter of the pupil should suffer in different degrees.¹

It has been necessary in dealing with this somewhat intricate subject to use a number of terms some of which can scarcely be said to be in common employment. It may be convenient, even at the risk of some repetition of what I have just said, to define these and to add a few words about the conditions severally designated by each.

Paralytic mydriasis is the condition of dilatation of pupil which occurs when the circular fibres are paralysed and the dilators intact. It may result from disease in the short root of the ciliary ganglion, or in the trunk of the third nerve, or in the brain itself. When present alone it may be presumed to be due to disease in the short root. As an uncomplicated condition it is very rare.²

Paralytic myosis is the condition of contraction of the pupil which occurs when the radiating fibres are paralysed, the circular ones remaining intact. It results from disease of the vaso-motor root of the lenticular ganglion or from disorganisation of the trunk of the vaso-motor nerve in the neck, or from disease or injury to the brachio-cervical region of the spinal cord. It is seldom in such a case that the pupil is contracted in any marked degree, unless the circular fibres are thrown into spasm: a condition of simple inability to dilate but without any special contraction is the common result of paralysis of the vaso-motor. As a consequence of

¹ Is it not reasonable to suggest that the disease of ganglion acts at first by simply weakening the nerve currents which traverse it, and that a current which may be too weak to reach to the iris may still get as far as the ciliary muscle?

² When mydriasis occurs with cycloplegia, and without other complication, it may be conjectured that the third-nerve root of the ganglion is diseased. I have notes of several cases in which this diagnosis was given. I have never seen both eyes affected.

injuries to the cord or to the sympathetic trunk in the neck paralytic myosis is not uncommon, and is usually combined with narrowing of the palpebral fissure and retraction of the eyeball. When not so complicated it may probably be due to disease close behind the ganglion.

Irido-plegia is a term applicable to total paralysis of the iris, both of its circular and radiating fibres. It is usually an indication of disease in the lenticular ganglion, and is then commonly combined with cycloplegia. It may at times occur from disease in the centres, but when such is the case other combinations might be expected.

Cycloplegia denotes paralysis of the ciliary muscle, and is expressed by loss of power of accommodation. In young and middle-aged persons it is easily appreciated, the necessity of a strong convex glass for reading (+ 10 or + 12) being conclusive. Cycloplegia occurs often, without combination with other ocular paralysis, after diphtheria, but in all other conditions it is rare excepting with paralysis of the iris. It does not occur, or at any rate is not complete, when the vaso-motor trunk in the neck is paralysed. We may presume that the ciliary muscle is supplied almost solely by the third nerve, but perhaps partly by the vaso-motor.

Ophthalmoplegia interna is a term which may perhaps be conveniently used to indicate a state of paralysis of all muscular structures within the eyeball (both sets of fibres in the iris and the ciliary muscle), and excluding all the external or orbital muscles. It is the symptom, or combination of symptoms, with which the present paper is concerned, and when it is present without any addition I claim that in all probability it denotes disease located in the ciliary ganglion.

A few words, in conclusion, on the general bearings of the subject may perhaps be permitted. Pending the results of post-mortem dissection it must still remain doubtful whether the group of symptoms which I have described is due to disease in the peripheral ganglion or in some central nucleus the position of which is not yet known to anatomists. If we accept, as I think we may safely do for the present, the

hypothesis that the ganglion is its site, we have, then, tolerably conclusive evidence that it is possible for syphilis (and perhaps other causes of disease) to attack and disorganise the separate ganglia connected with the vaso-motor system. If the lenticular may be thus involved why not Meekel's or the otic, why not any one or any pair of the cervical or thoracic ganglia? Our task in the future must be to seek out and identify the groups of symptoms which diseases of each of the ganglia separately or of many together would produce, and it may easily be that here we shall find an explanation of some symptoms which have as yet been regarded as anomalous.

Another line of investigation in the future will be as to whether when disease has begun in a peripheral ganglion there is much tendency for the morbid process (probably a neuritis) to travel along the nerves and involve other trunks or even reach the central nuclei. In two of my cases something of this kind appeared to have happened, for the patients who began with ophthalmoplegia alone had subsequently paralysis of the third. In none others was there any tendency shown to invade other nerves, but as already stated it must be remembered that all the patients were treated with specifics. There is another group of cases which I hope to have the honour at an early opportunity of bringing under the notice of this Society in which all the external muscles of both eyeballs become either in succession or simultaneously partially paralysed, and for which I shall venture to propose the name *ophthalmoplegia externa*. This affection is usually due to syphilis, and in it probably the centres are affected. It curiously and unexpectedly happens that in it the *internal* muscular apparatus of the eye often escapes. In one case, however, which is included in the appended table, the internal paralysis in a complete form preceded that of the external muscles, but as a rule it is remarkable that the two affections, although due to the same cause, keep distinct.

A contracted and sluggish pupil is a well-known symptom of locomotor ataxy, and often an early one. It is probably caused by defect in the vaso-motor fibres, and is not usually

attended by failure of the ciliary muscles. In one or two of the cases which I have mentioned symptoms were present which suggested the possibility that ataxy was threatened, and in connection with this subject we must recollect that several French observers, M. Fournier most prominently, have adduced facts in favour of the belief that this malady is in many cases due to syphilis. My own experience quite supports M. Fournier's statements, but it is to be remembered that to both of us probably only special forms of ataxy have been presented. Whether or not ophthalmoplegia interna is often met with in association with ataxy must be left for further investigation, but the facts as at present known would not favour such a suspicion.

I must finally add a word of apology for the endeavour to introduce a new term into use. I am well aware of the inconveniences of a loaded nosology, but as I feel confident that the group of symptoms described is a definite and important one I feel justified in suggesting for it a distinctive name. Whether this course is desirable and whether the proposed name is a good one must be left to the decision of the future.

A tabular statement of the cases which have occurred in my experience is appended overleaf.

Table of cases of ophthalmoplegia interna.

No.	Name.	Age.	History as to syphilis.	One or both eyes.	Eye first affected.	Remarks.
1	Mr. C—,	34	Mild syphilis some years before	Began in one and afterwards affected both	Left	Iridoplegia preceded cycloplegia in each eye. (Notes extend over two years.)
2	Mr. E—,	35	No history of syphilis	One	"	Iridoplegia preceded cycloplegia. (Only a single observation.)
3	Mrs. T—,	44	Syphilis four years before	Both	Simultaneous	Iridoplegia and cycloplegia coincident.
4	Elizabeth B—, single	27	No history of syphilis	Left complete, right threatened	Left	Slight improvement under a course of iodide.
5	A gentleman	35	Ditto	One only	Right	The condition had persisted eight (? 18) years. Some weakness of third-nerve recti.
6	Mr. H—, Veterinary Surgeon	36	Had had syphilis eight years before	Both	Uncertain, perhaps simultaneous	Iridoplegia complete; cycloplegia partial and somewhat variable under treatment.
7	Mr. S—,	37	Had suffered severely from syphilis	One only	Right	Recovered from cycloplegia, the iridoplegia remaining.
8	A sailor	..	Syphilis mildly years before	Both	Simultaneous	With paralysis of all the eye-muscles. The lenticular paralysis had probably preceded that of the recti by several months. (I have not mentioned this case in the body of the paper.)

Case of proptosis, first of one and then of the other eye, in association with enlargement of various glands.

By JONATHAN HUTCHINSON, F.R.S.

MR. S—, a Hindoo barrister, came over to England in September, 1882. I saw him in consultation with his brother, who was a surgeon, on the day after he landed. His right eye had been lost by inflammation after an operation for the removal of an orbital tumour, and his left eye was now in a condition of extreme proptosis. The lower lid was everted, and the whole of its mucous membrane exposed. The prominence of the eyeball and the eversion of the lid were very much greater than they are shown in the photographs, as these were not taken until about two months after the commencement of treatment.

Mr. S— believed that the condition of his left eye was now almost exactly similar to that of the right at the time the operation was performed. It is to be noticed that the proptosis was decidedly downwards. The movements of the eye were not much interfered with, but there was much conjunctival congestion and chemosis. The edge of the lacrimal gland, greatly enlarged and very firm, could easily be detected on a level with the upper margin of the orbit. On careful examination I could not feel certain of the existence of any definite tumour-growth in other parts of the orbit.

The upper eyelid hung rather loosely, but it was of course quite impossible to close the lids. There was a general puffiness of the whole of the face, especially in the parotid region and under the jaw. The subcutaneous

cellular tissue and fat being abundant definite examination was rendered difficult.

Mr. S— brought with him an excellent narrative of his case, written out by Mr. Cayley, of Calcutta, who had attended him there, and by whom the operation had been performed. It appeared that Mr. S— had always had full eyes, and that about three years ago it was for the first time noticed that his right eye was rather more prominent than the other. There was, however, nothing that was inconvenient or unsightly until about two years later, when the prominence had very greatly increased, and a firm tumour could distinctly be felt in the region of the lacrimal gland. Subsequently another growth was recognised in the lower part of the orbit. There was little or no pain, and Mr. S— was in his usual health. By degrees the eyeball was so much pushed out that the lids would not cover it. Mr. Cayley's notes state that the cornea had begun to look steamy, and the sight was somewhat affected. In April, 1882, an operation was performed, and the external canthus having been freely divided, the lids were dissected up and down, and first the lacrimal gland removed and next a firm lobulated mass, which occupied the outer and lower part of the orbit and adhered firmly to the periosteum. As far as could be ascertained, the whole mass was got away, and without injuring the eye or its muscles.

For a few days after the operation sight remained good, but eventually suppurative inflammation of the orbit ensued, the eyeball was again pushed out, and the cornea sloughed. After this the remains of the eyeball collapsed and receded, no fresh growth in the orbit taking place. The tumours removed were examined by Dr. MacConnell, the pathologist of the Calcutta Medical College, who described the one as glandular and developed from the lacrimal gland, the other as composed of fibro-adipose tissue, the fibrous elements preponderating; ("delicate, nucleated, fibro-elastic filaments").

Such was the history of the eye which had been first

affected and which was now lost. It is to be especially noted, as bearing upon the nature of the new growth, that there had been no recurrence, the condition of the parts in the orbit being much as is usual after suppuration of the eyeball from any other cause. At the time that the operation was performed there was no reason to suspect anything amiss with the left eye, but within a week Mr. S— was alarmed by finding that it was taking on exactly the conditions which had been observed in the first stages of the right. It became prominent for a time and then receded, and then became prominent again. He now determined to come over to England for advice, and left Calcutta on July 22nd. During the voyage the proptosis very greatly increased.

The condition of things at the time that he landed I have already described. It was sufficiently alarming, and Mr. S— was in great distress, regarding the loss of his remaining eye as almost inevitable. On the most careful examination I could detect nothing in his general health nor elicit any facts in his personal or family history which threw any light on the nature of the disease. He had worked hard in his profession, but had always enjoyed fairly good health. The proptosis, although attended by some difficulty in breathing, had not been associated with headache, nor as yet with any material diminution of sight.

As the result of the operation in the other orbit had not been encouraging, I decided to try, for a time at any rate, other measures. Mr. S— was admitted into Fitzroy House, a hospital home, and was treated by the sedulous application of ice over the forehead, the eye itself, and to the back of the neck. He also took six grain doses of iodide of potassium. Within a week a very considerable improvement had taken place; the eyeball was less prominent, the swelling of the conjunctiva less, and the lacrimal gland not so easily felt. The same measures of treatment were persevered with for about a month, when the recession of the eyeball was such that he could

close the lids. It was at this stage that the photograph was taken.

There was still, however, a roll of everted mucous membrane visible below. At this time Mr. S— was allowed to go out, and the use of the ice was much interrupted; for a week also he omitted the iodide. A relapse took place, and the border of the lacrimal gland again came prominently forward. I now suggested a consultation with Mr. Bowman and Mr. Nettleship, and this took place in the early part of November. It resulted in an increase of the dose of the iodide and the addition of small doses of mercury.

It should have been stated that before this I had discovered that the edges of the parotid gland on each side could be distinctly felt, projecting as a firm lobulated mass forwards on the masseter. The edge of these glands, although not quite so hard, was to the touch remarkably like that of the lacrimal gland. There was also some enlargement of the lymphatic glands on both sides of the neck. The result of the more vigorous treatment was as satisfactory as it had been in the first instance, and my note on December 6th states that the eye had receded so that he could again quite close the lids, that the lacrimal gland could be discovered only by deep pressure, and that both the parotid and the lymphatic glands were very much reduced in size. Mr. S— at this time appeared quite well and was accustomed to take much exercise.

A month later he returned to Calcutta. The eyeball at this time had receded almost to its natural position, but there was still a narrow rim of everted mucous membrane visible between it and the lower lid. He was to continue the iodide and increase the dose if threatened with relapse.

In attempting to investigate the nature of this remarkable case, we must remember that amongst the conditions which were demonstrable was the enlargement of glands of three different functions, a salivary gland (the parotid), the lacrimal gland, and the lymphatics of the neck. In

each instance the enlargement was firm and fleshy, quite painless, and without tendency to inflammation. The increase in size of the lacrimal gland, although coincident with the proptosis, certainly did not cause it. The gland overhung the eyeball and was moveable on it. We are driven therefore to the belief that there must have been some swelling of the fibrous or fatty contents of the orbit which caused the prominence of the globe. Whatever it was it appeared to be capable of spontaneous diminution and prone to relapse, and to be, as well as the enlargement of the several gland structures, definitely under the influence of the iodide of potassium.

Remembering the degree of recovery which took place and the fact that there has been no fresh growth in the right orbit, it is not possible to entertain the suspicion of malignant disease. My impression is that the case should be placed in the same group with certain rare examples of the symmetrical formation of ill-defined but more or less lobulated masses of fibro-fatty tissue in the region of the neck. This affection was, I believe, first well described by Brodie. I have seen several examples of it, and Mr. Morrant Baker has reported a series of cases in the 'Transactions of the Clinical Society,' and has very carefully investigated its nature. In several cases portions of these tumours have been excised and demonstrated to consist of fibrous and fatty tissue. But in at least one case under my own care there appeared reason to suspect that the case was complicated by adenoma, death ensuing with symptoms of intra-thoracic disease. I have never as yet in any of these cases observed the symptom of proptosis, nor witnessed enlargement of the lacrimal gland, but in a case which was sent to me recently by Mr. George White, of Hackney, the parotids were enlarged in exactly the same way as has just been described in the case of Mr. S—.

In this instance, the patient, a man æt. 40, had huge symmetrical masses on the back of his neck and under his jaw, whilst in each forearm near the elbow there were

several of the common subcutaneous fibro-fatty tumours. I show a photograph of this patient.

I excised a portion of one tumour and proved that it consisted of fat. It is to be clearly recognised that in these cases the condition is one not of new growth, but of hypertrophic development. The fatty masses are continuous with the subcutaneous fat. They are, however, remarkably local and are not coincident with any marked tendency to general obesity. In the case first mentioned they were associated with isolated fatty tumours in the forearms.

It seems probable that we ought to widen our views of this group of affections and not too much restrict conception of its features to the typical and more common cases described by Brodie and Mr. Morrant Baker. In these the patients are almost always men, and the fatty outgrowths occur at the back of the neck. In women, if I mistake not, there is a parallel affection in which the fat accumulates not at the nape, but deep in the root of the neck. The cases which I have cited prove that in some instances there is with the tendency to local fat hypertrophy a liability to increase in size of glands. Perhaps on closer examination we may find that this conjunction is the rule instead of the exception. Very probably the apparent increase in the size of the glands is due rather to overgrowth of interlobular fat and cellular tissue than to increase of gland elements. This would explain why we find glands of very different functions simultaneously affected. There may be cases—and I think I have seen some—in which general hypertrophy of glands of the same nature occurred without any form of fatty outgrowth; for example, symmetrical hypertrophy of the parotids. The physical cause of the proptosis in Graves' disease is not well understood, and in many cases it appears to be in part at least hypertrophy of fat. This singular malady may possibly be a member of the same family group.

It is to be noted that some of the cases of the Morrant Baker type are accompanied by very marked disturbance

of nerve functions. The man whose case I have mentioned was excitable to the verge of insanity. It is to be noted further that these fatty outgrowths are liable to remarkable alterations in size in connection with the state of health and mode of life of the patient. Mr. Baker has observed that they usually happen to be heavy beer drinkers and are benefited by abstinence. I can corroborate this observation, and may add that I have known definite reduction of size from change of air from London to the country.

I fear it may be thought that I have entered upon a disquisition which is surgical rather than ophthalmic. It must be remembered, however, that my aim has been to discover the nature and probable cure of a malady which is special so far that it leads to destruction of the eyes, but which probably in all its relationships outsteps the domain of the ophthalmologist.

(July 3rd, 1884.)

DISCUSSION
ON
THE PATHOLOGY OF SYPHILIS.

February 1st, 1876.

MR. JONATHAN HUTCHINSON opened a Discussion on the Pathology of Syphilis. He said:—In engaging in a discussion on syphilis, with a view to the advance of our knowledge of it, there are certain facts as to its nature which we may, I presume, take as generally accepted. It is, I suppose, now a matter of general belief that the phenomena of this disease, notwithstanding their great variety in detail of character, are due to one virus, which, having been introduced into the body by contagion, developes within it,—with tolerable uniformity as regards stage and time,—certain processes of inflammatory growth. The pluralistic doctrine of Carmichael never obtained any wide acceptance; and it is now, I think, generally acknowledged that what was called dualism had not really any claim to such a name, since but few ever believed that there were really two rival forms of virus capable of producing constitutional effects. Further, even in its much more restricted sense, as implying that there are two distinct kinds of virus, to which are attributed two different forms of chancre, I think we may say of dualism that it is dead, and that the far simpler creed which attributes the soft chancre to contagion with inflammatory products produced by syphilis, but not, as a rule, containing its germs, is the one which now obtains general acceptance. We have, then, in syphilis, but one malady and one virus.

The objections which, on account of its apparent irregularity, were at first urged against the claim of syphilis to take position amongst the specific fevers, have been greatly diminished of late

by facts from two different directions : first, by the observation that the short-lived exanthemata are by no means so regular and uniform in their career as it had been customary to account them ; and that, in point of fact, they do vary quite as much as to length of stage, degree of severity, and occasional omission of some of their phenomena, as does syphilis. Secondly, we have come to appreciate more correctly the antidotal power of our specifics, and have remembered that it is not fair to allege against a malady that it is irregular in its course, when our observations are made on cases in which we have done our best, by means of a most potent antidote, to arrest its career. I have no doubt that, if mercury were entirely put aside, we should soon see that syphilis is quite as regular in its stages as variola, and also that it varies quite as little in its degree of severity in different persons. When mercury does not cure, it delays ; and the retardation of stages sometimes witnessed under its use is very remarkable.

Thus, then, what I shall have to say this evening will be based on the assumption that in syphilis we have to deal with a specific fever of prolonged but definite stages, which is produced by contagion only, which has a period of incubation, a period of outbreak (known as primary symptoms), and a period of efflorescence or exanthem (known as the secondary stage) ; and which, in exceptional cases, differs somewhat from its more short-lived congeners by being followed by sequelæ to which we give the name of tertiary symptoms.

Had it not been for this unfortunate tendency to recur and become protracted, syphilis might have remained a surgical malady. If it always ended, as it ought to do, with its exanthem or secondary stage, there would have been no need to trouble physicians as to its treatment, nor is it very probable that the Pathological Society of London would have thought it worth while to invite a detailed discussion of its life-history problems. To the annoyance and misery of thousands, however, syphilis does not always end with its apparent death at the expiration of the secondary stage. The patient seems then to be well ; he loses his fever ; his rash disappears ; his throat heals ; his hair grows again ; and his blood is renovated ; but he is not safe. There remains ever after a risk—comparatively a very small one, but still sufficient to cause much anxiety—that he may become troubled with a tendency to the growth of tumours, which will be in direct relation to his bygone

disease. These may occur in important parts, may attain large size, and will probably show very little tendency to spontaneous cure. To these we give the name of tertiary symptoms or sequelæ; and we recognise a most important distinction between them and those of the earlier stage, in that they are never general, and only by accident symmetrical. They do not constitute another stage of a blood disease; but, by their constant non-symmetry, appear to prove that now, at least, the blood is not concerned. We no more witness returns of the syphilitic exanthem—a symmetrical and general eruption—at long periods after the first, than we do recurrences of the smallpox eruption after the patient has recovered. Most unhesitatingly may we characterise as errors in observation a certain small number of cases which have been supposed to illustrate this occurrence. In all probability syphilis ceases to be a blood disease at the date when symmetrical manifestations cease to be usual. The precise period at which this takes place will vary with the treatment pursued and the idiosyncrasy of the patient.

The correct determination of the period at which syphilis ceases to be a blood disease could, perhaps, be accomplished with certainty only by inoculation experiments. That there is a period at which it does so cease without in the least exempting the patient from the risk of fresh local symptoms, appears highly improbable. Whilst the secretions from all forms of secondary ulcerations and the blood of patients in the secondary stage are abundantly proved to be contagious, the negative evidence as regards both in the tertiary stage is very strong. The secretion from a phagedænic sore in a tertiary case may spread phagedæna, but will not spread syphilis; and hence, probably, one frequent source of what are called soft sores. It would be rash to assert that it is impossible for tertiary syphilis to prove contagious, but at present I know no evidence which would support the affirmative. The test of contagion is probably the best that we possess as regards the existence of a blood taint, and guided by it there seem strong reasons for suspecting that risk of hereditary transmission may persist long after the cessation of blood contamination. At any rate, the risk of contagion appears to cease long before the risk of hereditary transmission. I admit that there is difficulty in conceiving of this, or in understanding how a taint no longer active in the parent's fluids can be reproduced with virulence in his offspring, but the facts seem to point in that direction. In speaking of the possibility of local

syphilitic phenomena occurring long after the cessation of blood-taint, it is intended to imply very definitely the belief that many of the late phenomena of syphilis are purely local; and that, if they exist in parts which are accessible to local remedies, they do not imply any necessity for internal treatment. If I may be pardoned a statement which refers to clinical rather than to pathological matters, I may say that the long persistent and frequently relapsing sores on the tongue, and many forms of skin diseases, palmar psoriasis, lupoid tubercles, &c., are local and not constitutional, and are cured quite as easily by local as by internal treatment. The internal gummata are also probably often only local, but you cannot bring mercury or iodine into contact with them, excepting by introducing these remedies into the blood. It is scarcely necessary to remark that success by internal treatment by no means proves that the disease was more than local.

Before proceeding further to speculate upon the true relationship between the secondary phenomena and their sequelæ, it may be best to glance briefly at some of the peculiarities of the syphilitic inflammations. So peculiar are the products that one is almost tempted to speak of new growth rather than of inflammation. From beginning to end—from the chancre to the latest tertiary gumma—the tendency to cell-growth is most remarkable; and the production of a solid palpable mass, often very firm, is a characteristic feature of the inflammatory process when lighted up by the virus of syphilis. It is, however, by no means the only one. An avoidance of proclivity to suppuration (not constant, but still very marked), a tendency to cause death of the tissues affected, and thus produce phagedænic ulceration, or even sloughing, and a proneness to undergo rapid and complete absorption, especially when attacked by certain metals or their salts, are features which characterise and distinguish the new growths due to syphilis. Let us add that, whilst absorption may easily be procured, relapses are very prone to occur, especially if the treatment be arrested too soon. It is to be insisted that these peculiarities are the common property of syphilis in all its stages. A gumma in the tongue may be almost as hard as a chancre, and either of them may be, and have been many a time, mistaken for scirrhus. A chancre, when very large and dense, may perish and slough out just like an overgrown gumma. The chancre, the secondary rash, and the gumma are all alike liable to undergo molecular death and to be attended by

phagedænic ulceration. To attempt to distinguish between the secondary and tertiary stages, by saying that the latter only is attended by liability to deep ulceration, is to set facts at defiance.

The attempt to found distinctions as to the stage of syphilis upon the tendency or otherwise to deep ulceration, has been the means of introducing much confusion into the subject. It cannot be too strongly insisted, that this may be shown at any stage. It may be remarked also, in passing, that phagedænic action constitutes an exception to the statement that the inflammations due to syphilis in an early stage show a tendency to spontaneous cure. When the wizard Syphilis has once called up the demon Phagedæna, it has evoked that which it is powerless to control. No law of spontaneous cessation and recovery will now be observed; nor will the specifics for the parent malady avail much as regards the offspring. Phagedæna, once started, exists on its own behalf, and spreads by the contagion of its own pus. To arrest its spread it is necessary to destroy its secretion.

So marked is this liability to phagedæna in syphilis, and so rare in connection with any other cause, that with a few exceptions we may count syphilis, either directly or indirectly, as the parent of all phagedæna. The discussion as to whether rupia and its allies are secondary or tertiary symptoms may thus be easily decided, by explaining that there is nothing in the "rupial sore" characteristic of either, and that its features may be assumed in both. If rupia occur as a symmetrical eruption, it will always be found that the period which has elapsed since the contagion is only short, and that the disease is consequently secondary; but if it be unsymmetrical, then in all probability it is tertiary. Histologists, I believe, admit that the differences in cell-structure between an indurated chancre, a secondary tubercle, and a tertiary gumma, are only very slight, and that essentially all three are constructed on the same plan.

Thus, in examining the question as to the relationship between the several stages of syphilis, it is, I think, a matter of necessity that we admit that many features and many tendencies are shared by the phenomena which occur in all. Whilst, however, their similarities are marked, so also to some extent are their differences. First, and as by very far the most important of the differences between the stages, we place one which has been already mentioned, the tendency to general and symmetrical development in the secondary stage, and to local, restricted, and unsymmetrical formations in

that of sequelæ. This, however, after all is merely a question of abundance and not so much of character. In the secondary stage the blood and all the tissues are involved ; whilst, in the later ones, only certain regions, or it may be only single spots, are affected. The second difference is, that a spontaneous tendency to resolution of the new growths and to absorption is constantly witnessed ; whereas it is exceptional in all tertiary products. It is, however, by no means certain that spontaneous disappearance does not often occur in the case of tertiary growths. Still it may be fairly granted that a proneness to persist, to grow, to spread, and to contaminate adjacent parts, is far more frequently witnessed in the tertiary than in the early formations. I am puzzled to find for mention any other characteristics which distinguish these two classes of symptoms. It may probably be suggested that there is difference as to the parts liable to be attacked, and as to the depth to which ulceration may go ; that the symptoms in the secondary stage are all superficial—skin and mucous membranes—and that they do not spread deeply ; whilst those of the tertiary stage are apt to occur in cellular tissue, periosteum, bones, viscera, or meninges. I can, however, only admit with great limitation these assertions. Deeply spreading ulceration occurs not unfrequently in the secondary or even in the primary stage ; and as to the immunity of the viscera, cellular tissue, and periosteum in the secondary stage, it is far more easily asserted than proven. Some able writers on syphilis have proposed the term “period of gummata” as a substitute for “tertiary stage”—but really gummata may occur in the secondary stage also. It is true that the large gummata of the testis, cellular tissue, liver, &c., are rare, excepting in the tertiary periods, and hence a general impression on the subject, which is, I suspect, very false. But it is not absolutely true that even the large gummata are not seen in the secondary stage. Abundant facts on record disprove such an assertion. Distinctions which relate only to size may easily mislead.

The visceral pathology of the secondary stage might form a chapter in the history of syphilis which has not as yet been written, and for which we possess but few data. It is, however, I feel sure, a great mistake to suppose that there are none to be obtained ; and this Society would, I think, be doing a most useful work in encouraging the production of carefully sifted evidence on this point. It is, of course, rare for patients to die during this stage, and

post-mortem examinations are not frequent. The widely spread belief that secondary syphilis, in the acquired form, has no internal pathology worth working at, has probably led to the waste of not a few opportunities which have chanced to occur. I venture to foretell that, when the facts are forthcoming, we shall change our opinions on this point very definitely. There is surely no *à priori* probability in the creed that a blood-disease so severe as syphilis should produce lesions on the skin, in the mouth, and in the eye only; that it should, in fact, affect all the visible parts, and avoid all the concealed ones. The fact that the latter are concealed is probably the only reason why we believe so. The real difference between the secondary and tertiary manifestations, let me repeat, concerns probably some minor facts as to their course and tendencies rather than the parts attacked. In the secondary stage all the morbid processes are transitory, and tend to spontaneous cessation; whilst the tertiary ones are more persistent, and tend to destruction of the parts concerned. But, apart from mere inferential conjecture, there are certain facts already ascertained which seem to discredit the opinion that certain tissues are attacked in the secondary stage, and certain others in the tertiary ones. Time will not permit my mentioning cases, but I may briefly enumerate the following points.

1. In the inflammations of the eye well recognised as early and secondary we have various tissues involved: the iris, which is fibro-muscular; the hyaloid, the retina, and usually, I think, at a somewhat later period, the choroid. In the iris, and in the choroid, little tumours, analogous, I suspect, in all respects to gummata, are seen.

2. When the internal ear is attacked in acquired syphilis, a very rare event, it is usually during or immediately after the secondary symptoms.

3. In two cases of death from syphilitic disease of the heart which have come under my own notice, the event in both occurred in the secondary stage (myocarditis with gumma).

4. In one of the latter cases (examined *post-mortem* by Dr. Sutton) definite gummata were found in both testes and in the spleen, although the secondary rash was still out in the patient's skin. A similar case in an infant, three months old, with secondary rash, has recently been brought before this Society by Dr. Coupland. Parallel facts have been recorded by French observers.

5. It is very common for patients during the secondary stage to complain of symptoms which would imply disease of the same tissues which are attacked later on in the tertiary period. Rheumatoid pains often with slight swellings over bones represent probably the nodes of a later stage. Febrile disturbance, severe headache, loss of appetite and strength, are all symptoms which the eruption on the skin scarcely suffices to explain. Transitory albuminuria is not uncommon, and some authors assert that jaundice may occasionally occur. We should probably see more of such symptoms were it not for the common practice in England to assail the disease with its antidote.

6. The strongest reason for believing in the frequent occurrence of visceral and periosteal lesions in the secondary stage of acquired syphilis is, that they are very common in the corresponding stage of the inherited disease. In the latter the secondary stage is far more severe than it is in adults, and often ends in death, and abundant opportunities are afforded to the pathologist. In infants, it is well known that visceral and bone lesions occur not very infrequently and simultaneously with the skin-eruption. The best example of gummata in the liver that I have ever seen was from an infant. Mr. Canton has described in our 'Transactions' a case in which these growths attained a large size, and showed all the characters which are displayed in those of the adult. The able work of Dr. Taylor, of Boston, just published, contains abundant evidence on the same point. To Wegner, of Berlin, we are indebted for important investigations proving the frequency of periostitis in infants; and when syphilitic sarcocele (gumma, and often large) occurs in inherited disease, it is always in young children and sometimes in infants. Nor is it possible to dismiss this class of facts by suggesting that many infants begin with the tertiary stage, and that probably those who show internal lesions are born of parents who have suffered from the disease long ago. It is, I believe, almost invariable for infants to begin with the secondary stage, whatever may have been that which their parents may have reached. And, further, in not a few cases in which infants have displayed phenomena ordinarily ranked as tertiary, they were the offspring of parents who had but recently suffered, and they themselves showed at the same time the usual symptoms of the secondary one. It is certain, then, that, in infantile syphilis at any rate, visceral and periosteal lesions often occur in the early

periods. Let it be added that precisely the same kind of differences are observed between the periostitis of infancy and the periostitis of some years later, that we see between the transitory bone-pains of the secondary stage in adults and the more lasting nodes which follow years afterwards. In the infant the epiphyses and adjacent bones swell and become tender ; but, after a while, all trace of enlargement passes off, and unless, as some times happens to a growing bone, deformity is caused, no trace of the malady remains. Five or eight years later, very probably the same child's bones may become thickened by permanent osseous nodes.

The theory suggested as to the relationship between the secondary and tertiary stages would amount to the abandonment of the latter as any true stage at all. I would emphasise the teaching of those who regard tertiary symptoms merely as *sequelæ*. Many authors, impressed with the difficulty of arranging in any orderly manner the various sequences of syphilis, have been willing to give up the use of the term tertiary ; but it has, I think, always been found necessary to substitute for it some other in no respect more satisfactory. In Lancereaux's able treatise we have the term "period of gummy deposits." My argument is, however, that gummy deposits occur also in the secondary stage, and that in this they differ only from those of the tertiary in that they rarely attain any large size, and usually subside spontaneously. The gummata of the early period are usually small—miliary as they have been called ; those of the tertiary sometimes attain a large size. On this admission of fact follows closely an hypothesis which may prove important. Are not the tertiary gummata probably regrowths in structures left behind from the secondary stage ? Such an hypothesis, if plausible, would undoubtedly help much to the simplification of the subject ; it might also in the future assist in giving definiteness to our plans of treatment. The ingenious doctrine of residual abscesses, ably propounded by Sir James Paget, may aid us in the appreciation of this suggestion. We need not, indeed, go far in any direction of pathological observation for proofs of the tendency of parts once diseased, or the site of morbid products, to relapse even after very long periods of quiet. An especially curious and apposite one is afforded by the rare cases in which indurated chancres relapse. The fact that indurations sometimes recur in parts where formerly chancres existed, without any fresh contagion, has been noticed by several writers, and, amongst others, Mr. Lee

has recorded an example of it. It has, however, I think, scarcely received the attention which it deserves as an illustration of pathological law. I have under occasional observation at the present time several patients who are subjects of the relapsing chancre. Indurations, in all respects like primary sores, occur repeatedly, and without any fresh contagion, and present features which would defy the most experienced observer to diagnose them excepting by the history. They may remain for a few weeks, or even months, and may occur many years after the patient has passed through his secondary stage. Mr. Lee has suggested that they are to be regarded as tertiary gummata; but it is to be noted that they are as hard as cartilage, never slough, often disappear spontaneously, and are always located exactly in the site of the original sore. It is this latter fact which seems to me so important in reference to the theory just suggested. It may be well to add that the facts that these relapsed sores are never followed by secondary symptoms, and that they may occur not once or twice, but half a dozen times to the same individual, conclusively support the patient's assertion, that they are not produced by fresh contagion. If an indurated chancre may, in virtue of something which has been left behind, develop afresh in its old site ten years after the primary disease, why may not a like event happen to the cell-products of the secondary stage? It is not to be forgotten that a difficulty may be suggested in reference to this hypothesis, in that the tertiary gummata occur often in parts which are not proved to have been involved in the secondary, and are absent in others in which they frequently are so. For instance, secondary iritis is common, but tertiary iritis or tertiary gummata in the iris are very rare. This objection is probably more apparent than real; and we must always keep in mind that tertiary phenomena of any kind are exceptional; and, further, that it is quite possible that regrowth is the most likely to happen where the original development has been but slight, and the cell-growth has been checked before its power was exhausted. It may, also, be fairly urged that, although exceptions happen, yet, as a rough rule, the tertiary neoplasms are met with in or near the parts which are most commonly affected in the secondary stage. The skin, the subcutaneous cellular tissue, the palate, and the throat, are their more frequent sites. It is true that the brain, nerves, muscles, and viscera claim our attention much more; but this is on account of the greater importance and

interest of the symptoms produced, not because they are really the most frequent sites of tertiary gummata.

In connection with this theory, that the tertiary growths occur in cell structures which have remained over in a quiescent state from the secondary period, it is matter of justice that I should mention the great name of Virchow. Amongst the many suggestions for which we are indebted to his labours, is his well-known theory that relapses of symptoms are caused by stores of poison which have remained latent in various parts, especially in lymphatic glands, and, undergoing development after an interval, cause fresh contamination of the blood. This, however, is not the same idea as that the tertiary growths result, not from blood-contamination in any way, but simply from local renovation of long-resting germs.

In contrasting the course of inherited syphilis with that of the acquired disease, several remarkable features at once arrest our attention. Amongst these are the severity of the secondary stage, (often fatal) and, on the other hand, the frequent omission of all early symptoms; the remarkably long periods of latency which ensue after the cessation of the infantile symptoms; and, lastly, the great rarity of most of the conditions, which in the acquired form we rank as tertiary. I am warned that I must be exceedingly brief in what I have to say on these topics, and to the first two I will not further allude. The periods of latency which are often witnessed constitute, however, a phenomenon too remarkable to be passed over. As is well known, most syphilitic infants suffer during the first few months of life from affections of the skin and mucous membranes which are clearly analogous with the early secondary symptoms in adults. These pass off after a time, and at the end of a year, if the child has survived, it usually appears to be quite well. It may now remain for five, ten, twenty, or even five-and-thirty years without any further indication of its taint, and then may occur some definite and most peculiar affections. That there is a form of interstitial inflammation of the cornea, which when well characterised can be recognised as most certainly due to inherited syphilis, is a fact which, I believe, is not now doubted by any English observers. It is usually symmetrical, and, in a large majority of cases, it runs its course and disappears spontaneously, and as completely as does a secondary syphilitic rash. Nor are the facts as to the long period of latency which may precede it more in dispute than its own character, and I shall scarcely be blamed for

exaggeration if I say that they are amongst the marvels of pathology. Not only may this form of keratitis occur to a grown-up person who has had no symptoms of syphilis since infancy, but it may happen also to one who has never had any symptoms before, and may yet be itself most characteristic. It is difficult to offer any explanation of the cause of this long delay, other than that it has, perhaps, something to do with the gradual development of the tissues. In connection with it we must remark that, however long may have been the interval of latency, interstitial keratitis is still plainly one of the secondary group. Its symmetry and its tendency to spontaneous disappearance both prove its position in this respect, and we note, besides, that certain other phenomena which sometimes occur at about the same age show similar characters. The choroiditis, and the disease of the ear which in these patients so often lead to deafness, are both of them usually symmetrical, and so also very often is the more chronic form of periostitis. I do not mean that the symmetry is absolute. It is not the Dutch-gardener sort of symmetry, which insists on an absolute correspondence, both as to time and features, in the two halves ; but still it is sufficiently well marked to assure us of the fact. Very often one eye takes precedence by a short time of the other, and often one suffers more severely than its fellow. I have yet another and a stronger reason to urge for placing the keratitis and the inflammation of the internal ear, which are so frequent under the circumstances described, in the secondary class ; namely, that when met with in acquired syphilis they are distinctly secondary. Both are, then, very rare. Of the internal otitis I have seen but few examples, and of the keratitis but one ; all the cases, however, occurred to those who had but recently acquired their disease. Until within the last three months I have always been inclined to deny that the interstitial keratitis common in inherited syphilis had any analogue in the acquired disease. There is, however, at present in the Moorfields Hospital, under Mr. Wordsworth's care, a case which, as far as my own observation has gone, is unique. It is an instance of acute symmetrical keratitis in connection with acquired syphilis, and it occurs simultaneously with a general papular eruption, and without doubt within a few months of the primary sore. Thus it would appear to be a fact in hereditary syphilis, that conditions which in the acquired disease would occur within the first year, may be delayed for ten or twenty. Next let us note that the

conditions which chiefly attract attention in the tertiary stage of acquired syphilis are almost wholly absent in the subjects of inherited taint. Large gummata in the cellular tissue, in muscles, in the tongue, in the viscera, or in connection with the cerebral meninges, are almost unknown. The paralysis of single muscles of the eyeball or of those of the face; the attacks of hemiplegia and of optic neuritis, with which we are so familiar in subjects of acquired disease, are, I think, never seen in the inherited form. The able work of Dr. Buzzard on 'Syphilitic Nervous Affections' does not record a single example of such in connection with inherited taint; and although Dr. Hughlings Jackson has with his usual zeal availed himself of almost unequalled opportunities, he has succeeded in getting together but few cases—and none exactly of the kinds I have specified. It is true that epilepsy and some forms of idiocy and insanity, and some other anomalous spinal symptoms, are met with in heredito-syphilitic patients; but, I repeat, we have none of the cases in which symptoms are supposed to be due to gummata, whether of nerve-trunks, of the arterial coats, or of the coverings of the brain.

To the last-named author we owe most of our knowledge as to the discrimination of the different anatomical lesions which cause nerve-symptoms in syphilis. It is mainly in consequence of the elucidation of the subject due to his labours that I am enabled to make the statements which I now record.

It is clear that we have yet much to learn respecting the laws of hereditary transmission. As just remarked, it is exceedingly difficult to assign reasons why phenomena which in acquired disease are crowded together in the first year or two, should in the inherited form be separated by ten or twenty. Nor can we offer any explanation of the fact that in the late periods of the inherited disease there is no tendency to the formation of the neoplasms frequently seen in the acquired form. I said that the chapter describing the internal pathology of the secondary period in acquired disease had yet to be written, and I may now say the same as regards that of the tertiary period in the subjects of inherited taint. With but rare exceptions the latter do not appear to suffer from any maladies which tend to shorten life. I made this statement in print many years ago, and nothing has occurred since to induce me to modify it. Although I have had constantly under observation numerous adolescents and adults well known to have suffered

severely from inherited syphilis, neither in them, nor in their brothers or sisters, have I encountered any special disease of important organs. They have suffered in their eyes and ears, and sometimes in the skin; but here, for the most part, their liabilities seem to end. I do not think that I have made, or known made, more than half-a-dozen *post-mortem* examinations in such subjects, and in these for the most part no very noteworthy lesions were found. In two such death was preceded by long-continued albuminuria, and suspicion occurred that possibly the very long-continued use of iodide of potassium might have had something to do with it. So meagre is our knowledge of this department of the pathology of syphilis, that our Society would, I think, do well to encourage the production before it of all evidence which may in the future be obtainable, with a view to its permanent record in our 'Transactions.'

I now come to the last of the questions which is on my list for consideration, Is there any reason to believe that a taint of syphilis may mix itself with other causes of disease, and produce results of a hybrid nature? It is to serofula and serofulous disorders that chief reference is here made. Does syphilis in any way predispose to serofula or tuberculosis? Vague suspicions have been entertained on this head almost since the earliest date of recognition of constitutional syphilis; and opinions, often of a very sweeping tenor, have been expressed with great confidence by many authorities. Formerly, indeed, it was a question very difficult of investigation. I would venture, however, to submit to the Society that the facts which have been accumulated during recent periods, and the new symptoms which have been placed at our disposal, justify us in believing that syphilis produces in all its stages special and wholly peculiar lesions; and that although these may easily be mistaken for struma, they have in reality nothing whatever to do with that state of constitution. It is undeniable that syphilis, and sometimes the treatment required for it, may for a time enfeeble the system, and that during this state of debility a person predisposed to phthisis or struma may experience an increase of his proclivity. In this, however, there is nothing more than what is common to all causes of enfeeblement, whether special or otherwise; and I do not think that in the instance of syphilis it amounts to much. Syphilis may be contracted by persons of the most various states of health, and by those who are the subjects of special diathesis.

Strumous or tuberculous subjects, those who suffer from gout or from psoriasis, and those of extreme enfeeblement of circulation, may all in turn present us with examples of this specific disease. As a rule, we do not observe any modification of the one by the other. A common psoriasis rash may persist during the treatment of constitutional syphilis, and may remain for years afterwards, just as it did before ; and the same is, I think, true in the general way of lupus, leprosy, and other chronic maladies which show their chief symptoms in the skin.

In investigating the relationships between scrofula and syphilis we might conveniently take as type-examples of strumous manifestations two common and fairly definite maladies,—ulcers in the cornea, and Lupus. We have chiefly to consider the hereditary form of syphilis. A large majority of examples of chronic ulceration of the cornea (strumous ophthalmia) occur in those who show no signs of hereditary taint, but who, either in themselves or near relations, betray a tendency to other forms of scrofula. When such ulcers happen to those who are known to be the subjects of inherited taint, for the most part they run their usual course and require their usual remedies, while syphilitic keratitis is the same malady in the strumous and in the healthy. I do not say that this is invariable, but I think it is the rule. Respecting the common and interesting disease of the skin known as lupus, grave suspicions are still entertained by many who are well informed. To my own mind, however, the evidence is clear. True lupus has nothing whatever to do with syphilis either acquired or in the first or second generation of inheritance. The evidence on this point is of two kinds. In the first place, we hardly ever, as far as my experience goes—I might, I think, say never—meet with common lupus in those who are obviously the subjects of inherited taint. It must be granted, however, in the fullest manner that not all, or nearly all, of those who really inherit a taint betray it either in physiognomy, teeth, or by concurrent disease of suspicious character ; and, further, that it is precisely in those who do not so betray it that we must expect the history of symptoms in infancy to be wanting. In these cases, however, it is seldom that the patient is an only child, and it is very rare for a whole family to escape. If the surgeon will widen his inquiry, and, instead of contenting himself with the original patient, inspect carefully all the brothers and sisters, he will generally find evidence that is conclusive. In this way the

recognition of signs of inherited taint in one individual very commonly reveals its existence in possibly a quite latent form in three or four others. We identify not merely an individual but a whole family, and thus very much extend our area of observation. Making use of the symptoms of physiognomy, teeth, keratitis, and choroiditis, I do not think that it would be difficult in any large out-patients' institution to soon make the acquaintance of several hundred individuals concerning whom the observer might be sure that they all, in greater or less degree, inherited a taint of syphilis. Any one who had done this would be in a position to say whether lupus occurred in more than average proportion amongst this class ; and I have no doubt as to what his verdict would be. It is difficult to leave this part of our subject without suggesting that most valuable work in this direction, not only as regards lupus and scrofula, but in reference to the late results of inherited taint, might be done by those of our members who have the advantage to be engaged in family practice. If all the cases in which family advisers are aware of the fact, either of the existence of syphilis in parents or its actual presence in offspring, or both, would carefully note all that occurs to all the children in such families, not only in childhood, but in after life, a mass of facts might soon be collected which would help greatly to the solution of many important questions. It is far more difficult to follow out these inquiries in hospital practice.

There is yet another line of argument as regards the non-connection between syphilis and lupus. We have said that lupus does not occur frequently in the hereditarily syphilitic, or their brothers and sisters. Next, I assert that amongst those who have lupus and their relations, you will find, as a rule, not the slightest reason for suspecting specific taint. No one can, I think, doubt for a moment that lupus may occur under conditions which make the existence of syphilis either in parents or grandparents most improbable, nor, indeed, that it is, for the most part, under those conditions that we usually meet with it. Now, as lupus is a definite and peculiar malady, the probability is that it has a definite cause, and it is not due in one case to the tubercular diathesis, and in the next to that of syphilis.

There is no doubt that syphilis may produce skin-diseases which superficially very closely resemble lupus. This occurs most frequently in the acquired form of the disease, and in a few of these

the diagnosis is really very difficult. The essential difference in nature is, however, usually proved at once by the results of treatment. In inherited syphilis we seldom or never see anything which at all resembles the tubercular or common form of lupus. We do meet not unfrequently, however, with a disease which formerly was called phagedænic lupus, but which, like other varieties of phagedæna, is always and wholly the result of syphilis. It spreads far more rapidly than true lupus, and is easily curable by measures which would avail but little against the latter. Above all, it is easily distinguished after cure by the fact that its scars remain perfectly sound without the least tendency to the relapses which are so constant after cures of lupus.

Here, Mr. President, I must draw to an end. In attempting to fulfil the responsible duty with which the kindness of your Council has honoured me this evening, I have thought it better to avoid all subjects of mere detail, such as might seem to be more appropriate for special communications. I have kept two points chiefly before me: to try, if possible, to simplify and make more orderly our general view of the subject, and to direct attention to those parts of it which seem most to need further investigation. To my great regret I have, under compulsion as to brevity, found but little opportunity for mention of the names of those to whom we are indebted for the facts and suggestions of which I have freely availed myself, and of which, indeed, my paper is in some sort a running summary. Our knowledge of syphilis has, indeed, been "set in the light of many minds." If we were to pass over in silence all excepting those who have contributed recent additions to our stock, and of them take only those who have worked at the departments most interesting to this Society, the list would still be very long, and the difficulty of doing justice very great. In France, Ricord, Diday, Lagneau, Lancereaux, and Fournier; in Germany, Virchow, Wegner of Berlin, Dittrich of Prague, Von Bärensprung, and Zeissl; in Scandinavia, Bergh, Boeck, and Bidentkap; in America Bumstead—have taken foremost places; but their names do not even tithe those who have a claim on our gratitude. At home Dr. Wilks, I believe, led the way as regards visceral syphilis; and the earliest contributions were most of them made before this Society. Dr. Murchison, Dr. Moxon, Dr. Weber, Dr. Payne, have also contributed most valuable facts.

Our knowledge of the diseases of the nervous system, to which I believe Dr. Reid, of Belfast, was the first of our countrymen to contribute, was early helped on by Dr. Meadows, who, from the practice of the late Dr. Todd, published some cases in the 'King's College Hospital Reports;' and more recently by Dr. Hughlings Jackson, Dr. Buzzard, Dr. Russell, and Dr. Broadbent. Nor must I omit to mention, as zealous and successful workers at the general subject, the names of Lec, Berkeley Hill, De Méric, and Gascoyen; nor, last, the author of the most recent and, I think, most comprehensive and able treatise on the disease that has yet appeared—Dr. Bäumlér, whom we would gladly claim as, at any rate in training, half an Englishman.

The regulation of the discussion which is to follow, and the decision as to the precise scope which it shall take, will, I am glad to know, rest in much abler hands than mine. That I may not, however, fail of any part of my duty as its introducer, I have endeavoured to epitomise under the following heads some of the chief points raised in my paper. It will be seen that they are by no means closely connected; and that it will be quite competent for any speaker, if he prefer, to confine his attention to a single one, upon which he may be in possession of special facts, without adverting to the others.

Are there any facts which favour the belief that syphilis continues to be a blood-disease after the cessation of all tendency to produce symmetrical symptoms?

The dearth of facts illustrating the internal pathology of the secondary stage of acquired syphilis.

The importance of all facts or arguments for or against the belief that the gummata of the tertiary stage are purely local, and result from renewed growth in formations left over from the exanthem stage.

The value of facts as to the pathology of the late periods of inherited syphilis.

MR. H. LEE.—I believe, sir, it is a well-known fact that, when the sucker of a pump is a little dry at first, by putting a very little water in it you may bring a great deal out. In obedience to your command, therefore, I will make one or two short observations. In the first place, I quite concur with you, sir,—that is one point on

which there will be no discussion or division of opinion,—in thanking Mr. Hutchinson most sincerely for the scientific, careful, and talented way in which he has brought forward these several points. The area over which he has travelled is so large, and the number of subjects he has introduced is so great, each one of them being sufficient to occupy an evening in discussing it, that I think we must adopt his suggestion and consider them separately. The first point that struck me was that which Mr. Hutchinson first introduced about the blood-disease. Now, if we limit the term blood-disease to the stage to which he wished to limit it, certainly we want some better definition of a blood-disease. Mr. Hutchinson has described gummata in the liver at the same time that there were secondary eruptions on the body. The secondary eruptions he acknowledges to be a blood-disease, and we cannot but suppose that the gummata depended upon the same. We cannot think that the gummata depended upon one thing and the eruption upon another. At a subsequent part of his paper he describes gummata in a child's liver, and that, he says, is not a blood-disease. In the one case it would come under the head of blood-disease, and in the other case it would not be a blood-disease. I think, if I caught Mr. Hutchinson's observations aright, it was during the state of inoculability that he thought it was a blood-disease; but I cannot at all agree with that view. I should certainly think that if a father transmitted the disease to his son, that father must have had the disease in his blood in some way or other. Therefore I should consider all hereditary diseases as emanating from a blood-disease. Another point of considerable interest was touched upon by Mr. Hutchinson; that is, the way in which new matter is deposited principally in primary, but also in secondary syphilitic affections, and the way in which it is removed. Mr. Hutchinson classes all syphilitic affections under one head as derived from one poison; but I should have liked very much to have something more definite as to why one result is produced in one case and another result in another; whether it is something about the kind of inflammation that produces the inoculable product, or whether it is that in one case and something else in another. But, with the exception of phagedænic ulceration, Mr. Hutchinson, I think, has not given us any information upon these points. Certainly there are three distinct forms in which we can see the results of syphilitic inoculation develop themselves. There is an inoculation where we see new matter deposited perfectly

circumscribed, accurately defined, which does not tend for a long time, at least in a healthy state of the body, and perhaps not at all, to undergo ulceration or suppuration. That we call distinct infecting inoculation. In another case we see that instead of that the inoculable point suppurates at once, ulcerates at once ; there is no new matter deposited, and there is always a loss of substance, which there was not in the first case. There is a third form of inoculation (here I think I touch upon somewhat new ground) in which solid new matter is deposited which has not the characters that I mentioned in the first case, but it gradually diffuses itself into the consistency of the surrounding textures and has a tendency quickly to degenerate. That is what I think Mr. Hutchinson calls his soft sore. These are three very distinct forms of action, and they are not only perfectly characteristic, but, I believe, diagnostic of different affections. The first is the real infecting sore, which indicates the general infection of the patient's system ; the second is the suppurating sore, which always leaves a loss of substance, which always suppurates and ulcerates, and, as far as the evidence goes which I have been able to obtain, never affects the constitution ; then comes the third form, which has produced the confusion between the other two, where a person, having already had syphilis, is re-inoculated with that syphilis, and the induration has not then the period of incubation which is natural to syphilis acquired for the first time, but developes its results at once, and the newly deposited matter rapidly degenerates ; this form of disease afterwards simulates the soft sore ; but it is not a soft sore ; it has not run the same course ; it has had the solid matter deposited first before any ulceration takes place. In the first there is solid matter only, as the essential part of the disease ; in the second there is a loss of substance only, as the essential part of the disease ; in the third there is a combination of both ; and I think that, from not recognising this third form of syphilitic inoculation, the confusion between the other two has arisen.

Dr. CHARLES DRYSDALE.—So many points have been touched by Mr. Hutchinson that I can only allude to one or two. With very much that he has put forward I entirely agree, but one or two of his assertions seem to me to be capable of critical examination. For instance, the assertion that *Dualism* as a doctrine was dead was puzzling to me. If it had meant that the views of

Carmichael, of Dublin, that there were several varieties of the syphilitic virus, were no longer tenable, I should have agreed entirely to that; but he evidently spoke of the idea of the soft chancre being distinct from syphilis as being a defunct doctrine. To this I entirely demur. At any rate, both in London and in Paris, there are very many distinguished persons alive at this moment who are quite convinced that soft sores are not a part of the syphilitic drama at all. Thus, the hard sore has a long incubation, sometimes a very long one. I had a case the other day in the wife of a sailor who married in July, and whose husband left her in August, but whose sore did not appear until November, or three months, at least, after she was infected; whereas the soft sore has no period of incubation at all, as those know who have tried many inoculations, or have been present at Professor Boeck's wholesale inoculations, performed some years ago in London. The two diseases seem to me to be quite distinct, and, curiously enough, they are, at this moment, not often met with together in Paris. Thus, I have in my pocket a letter from Dr. Charles Mauriac, physician to the Hôpital du Midi, who informs me that, whilst hard sores and syphilis are more prevalent than they were ten years back in Paris, the soft sore is sometimes not seen at his hospital for weeks. I submit that, if Mr. Hutchinson be right, and the soft sore is only a product of syphilis (which he has not this evening proved), this fact mentioned by M. Mauriac would be hard, indeed, to account for; whereas if we believe, as I do, that the soft sore has always existed throughout history (for it was described by Celsus), and that the hard sore is a recent introduction in 1493, there is nothing mysterious if they should often coexist. I am inclined, with Mr. Hutchinson, to see a very great likeness between syphilis and the exanthemata; indeed, in some cases of syphilitic roseola I have noticed a fever with a temperature of 104° F. The incubation of the poison makes syphilis like smallpox; but the eruptive period of syphilis usually lasts about a year, or may even extend sometimes to twice as long as that, in very rare cases. I am almost inclined also to be seduced by the idea of a parasitic origin for the disease, as Mr. Hutchinson believes, and with this notion, and moved by his opinions and those of Dr. A. Fournier, of Paris, I have recently given small doses of green iodide of mercury for long periods in the secondary stage (one third of a grain twice daily for perhaps some ten months) with the idea of thereby

attacking the virus in the tissues, and thus warding off the tertiary stage. There are, however, cases which puzzle me greatly, for I have published one where a woman had no less than eleven dead births, one after the other; and in other cases syphilitic children seem to have been born more than five or six years after infection, which certainly does not look as if the virus *germs* had then left the system. I think that the mother of a syphilitic child has *always* contracted the disease in her system; although experience shows that sometimes syphilis is such a mild disease that scarcely any symptoms of it may be seen. The ordinary rule, however, is that a mother, in two or three years, begets quite healthy children again, as if the germs were then all dead. Women with even fatal forms of tertiary lesion sometimes give birth to fine healthy children. One woman who attended the Metropolitan Free Hospital for many years with sloughing tertiary sore throat, and eventually died of it, gave birth to two fine children, which had no apparent taint of the disease. In the secondary period, again, gummy deposits are not so rare as used to be thought. They are found in the testicle about the end of the first year; in the liver and in the brain, in some cases very early in the disease. A man came to my hospital with right hemiplegia and aphasia, some years ago, whose body was completely covered with the rash of syphilitic roseola; and I have several times seen enlargement of the liver with jaundice, whilst roseola was present. Another difficulty in admitting that the tertiary period is entirely free from the germs is that, occasionally, in traumatic cases, as shown by M. Verneuil, of Paris, rupial eruptions may arise very many years after the original infection under the influence of some operation. Again, nodes certainly appear on the forehead sometimes two or three months after contagion. But, as a practical rule, I confess I always tell patients with syphilis that they may marry without much fear of contaminating their children in a couple of years after the mucous tubercles, &c., are over, *i. e.*, about three years after primary infection.

When children get syphilis, their disease, in my experience, is usually mild when not inherited; such children do not often seem to suffer much from tertiary disease. With regard to Mr. Hutchinson's splendid discovery of the way of diagnosing inherited syphilis, by means of the two upper central incisors, it is a singular fact that, in London, this is now deemed by experts as clear as the

sun at noon-day, whereas, in Paris, many great syphilographers have their doubts about it; for instance, MM. Verneuil and Fournier. The place to see such cases is in a large hospital for eye-diseases, such as Moorfields. Nervous diseases are, I think with Mr. Hutchinson, very rare in inherited syphilis; but bone disease and other tertiary phenomena, with affections of the tongue, are not so very uncommon, and patients with inherited syphilis cannot, I think, often live to a very old age. Hereditary syphilis is quite distinct from scrofula, but syphilis causes a kind of pulmonary phthisis with cavities in the lungs curable by iodide of potassium. Women with hereditary syphilis give birth to healthy infants. I examined the scrofulous children at Margate and at Berk-sur-mêr, and saw no cases of hereditary syphilis among them. With regard to the treatment of the various periods of syphilis, experience has shown me that late on in the disease mercury is usually of scarcely any use. In tertiary sore throats, ozæna, and sloughing ulcers, large doses (10 grains or more) of iodide of potassium should be given. In rupia both iodine and mercury may be tried. It is only in forlorn-hope cases of tertiary disease that mercury need be given, in my opinion. Mercury, in short, for the germ period, iodine for the tertiary lesions.

Adjourned Discussion, February 15th.

Sir JAMES PAGET.—I desire to begin by expressing my agreement in the main with the doctrines of syphilis which Mr. Jonathan Hutchinson propounded at the last meeting of the Society. I agree with him in the belief, if he will allow me to quote his own words, that in syphilis we have to do with but one malady and one virus; that in syphilis we have to deal with a specific fever of prolonged definite stages, and that it is the sequelæ of the fever to which we give the name of tertiary symptoms.

Mr. Hutchinson has rightly indicated some of the points to which he would suggest that the discussion of the subject should lead; and I will take one of these, namely, the relation of the tertiary symptoms which he regards as the sequelæ of the syphilitic fever, if I may use the term. In this view I do not pretend to add any

new facts to those which Mr. Hutchinson has himself stated. If I can add anything to strengthen the arguments he has adduced, I think it can only be by pointing out the very close resemblance which exists between the tertiary symptoms and those which may be regarded as sequels of other forms of fever. The very characters which Mr. Hutchinson has pointed out as indicative of the tertiary period, the sequel of the syphilitic fever, are, first, that the symptoms which it presents are not symmetrical; then, that it is a disease which is not contagious, nor yet, so far as experiments have yet proceeded, distinctly communicable. Moreover, I suppose it may be added that it is very seldom transmissible by inheritance.

Let me take occasion to speak personally of my gratification that the doctrine of the symmetry of disease is upheld upon so strong grounds as those which Mr. Jonathan Hutchinson has introduced; for, so far as I know, the full significance of symmetry as characteristic of a blood-disease has scarcely been dwelt on since it was the subject of papers communicated to the Royal Medical and Chirurgical Society by Dr. William Budd and myself; and if, in speaking of syphilis as a blood-disease, Mr. Hutchinson has raised some strong questions in modern pathology as to whether the term blood-disease can, under any condition, be justly used, I will recommend those who object to the term to read carefully the paper by Dr. William Budd in the twenty-fifth volume of the 'Medico-Chirurgical Transactions.' I think they will find there something as near to demonstration as can be arrived at in pathology, that diseases which are symmetrical owe their existence to a morbid material in the blood; and that those which are regarded as the morbid products of the disease shown symmetrically are really the morbid material combined for a time with the natural texture of the parts affected. To that doctrine I still hold; and I may have occasion to refer to it in that meaning many times in what I say. The years that have passed—more than thirty now—have shown, doubtless, that the nervous system has a larger share in the determination of symmetry in disease than either Dr. William Budd or myself at that time supposed. Nevertheless, the main facts hold true, that, as Mr. Hutchinson has illustrated, a symmetrical disease does, by its symmetry, indicate the presence of a morbid material in the blood. Moreover, I would add to what he has himself demonstrated, that the product or, as we call it, the

appearance of the disease, is due to the combination of the morbid material with the natural textures of the parts.

Mr. Jonathan Hutchinson asks, as one of the first questions which he submits for the consideration of the Society—if I may again use his own words—Are there any facts which favour the belief that syphilis continues to be a blood-disease after the cessation of all tendency to produce symmetrical symptoms? Now, I think, that to answer that question we must inquire what are the characteristics of the sequels of other fevers in reference to symmetrical disease. It is clear that the sequels of all other fevers are, with very rare exceptions, not symmetrical. There are the sequels of pyæmia, the pyogenic fever; its phenomena, in its acute stage, are rarely symmetrical, but when it passes into its sequels in the chronic stage its phenomena are perhaps still more rarely symmetrical. They indicate a singular similarity to the tertiary symptoms of syphilis, in that they very commonly affect always the same textures; as, for example, a large number of them are shown only in suppuration in the connective tissue; or in other persons they appear only as slowly successive instances of necrosis, hercin indicating one of the most marked features of resemblance between the sequels of the best marked acute fever, and of that which some may deem the least marked—the syphilitic fever. But they are not symmetrical.

Again, intermittent fever is a fever well to be studied in its likenesses to syphilis, because it passes even beyond syphilis in the duration in which it will hold itself, as I suppose, in the blood of the patient. Thirty years are referred to as the time that syphilis may continue without intervening symptoms. It is no uncommon thing for ague to be repeated in its phenomena thirty years after the time of its first occurrence; but it is not notable that what may be regarded as the sequels, or the latest symptoms, of ague, are not symmetrical. They are best of all marked in the various neuroses, which are recognised under the terms of brow-ague, migraine, and others, for the most part not symmetrical affections of the nervous system.

Then I may instance the sequels of typhoid fever, a subject which, I venture to think, has not sufficiently attracted the attention of physicians, probably because the cases more frequently fall into the hands of surgeons. Within some years past I have been struck with the great variety of surgical sequels of typhoid fever,

and amongst them I have most rarely indeed met with instances of symmetrical disease. Among the instances of necrosis as the sequel of typhoid fever, ten or twelve in number, I have seen only one symmetrical—a well-marked case of symmetrical necrosis of the lower ends of both tibiæ; but in all others, whether affecting the tibia, the ulna, cranium, or other bone, there has been always an absence of any sign of symmetry. So in the case of phlebitis, which is common in the femoral vein as a sequel of typhoid fever, I have never seen symmetrical phlebitis. Nor have I seen a single instance of symmetrical affection of the nervous system as a sequel of typhoid fever, except in one or two cases, where complete paraplegia was produced; but in all those which are more common, paralysis of the anterior muscles of the leg, or of part of the muscles of the arm, or any other, I have not yet met with one instance of symmetrical paralysis. Lastly, with regard to an affection which is not unfrequently seen as a sequel of typhoid, suppurative periostitis of the ribs, six or eight of these cases that I have seen have shown no sign of symmetry.

Therefore, I think we may hold that in this absence of symmetry in these sequelæ, that is, in its tertiary symptoms, the syphilitic fever, if I may so call it, shows a remarkable feature of resemblance with all those other more acute fevers in which we can trace distinct sequels. And in this I suppose it only falls in with what is generally true concerning symmetrical diseases, or diseases of the blood, that in their later periods of existence they all become irregular, that is, they not only lose the regular character of symmetry as affecting both sides of the body alike, but they lose many of the regular characters of form and size and external appearance with which we are familiar. For example, if we watch lepra, or psoriasis, perhaps the best marked instance of symmetrical eruption, if after a time it spread from the limbs over the trunk, or from the upper extremities over the face, in its later periods it commonly ceases to be distinctly symmetrical. Or again, if I may cite an instance of what is not properly, I believe, to be called a blood-disease, yet is distinctly symmetrical (that is, the so-called pityriasis versicolor): it is symmetrical in all its early stages: when it is declining, it does not decline or fade with symmetry; or if it relapse, it does not relapse with symmetry. So with rheumatic gout or with cancer, in the condition in which I suppose, even in this room, it will be generally understood to be a blood-disease,

when it has thoroughly infected the blood in its later stages, and appears in multiple forms : these are not commonly symmetrical.

Therefore I think the rule may generally be, if we are to answer Mr. Jonathan Hutchinson's question as to whether there be indications that in the absence of symmetry a blood-disease has ceased, that the cessation of symmetry as a characteristic of syphilis is exactly accordant with its absence in the sequels of other fevers, and generally with its absence in the periods of decline of nearly all diseases that begin by being symmetrical ; in other words, in nearly all blood-diseases. And I apprehend that the reason of this decline may be assigned not only, as Mr. Hutchinson assigns it, to the influence of antidotal remedies, but, besides, to the constantly increasing prevalence of the natural nutritive forces over the morbid ones. For we may certainly hold that syphilis is a disease which does not usually tend to a continual increase and fatal termination ; that it is constantly exposed to the strife with the natural nutritive forces, which will not, indeed, simply diminish it in intensity, but, if I may so speak, gradually break it up by making it irregular ; and amongst the first signs of its irregularity make it not symmetrical.

But to revert to Mr. Jonathan Hutchinson's question, whether the absence of symmetry may be taken as the indication of the arrest of blood-disease, I must observe that in the hypothesis which follows in his paper, in which he assigns the occurrence of the tertiary symptoms to changes of structure ensuing in the residues of the secondary deposits, he would have to admit that in those residues the syphilitic disease shows itself as secondary, and therefore, as it should be, symmetrical ; and he would then have to explain why, being symmetrical in the forms of the secondary period, it becomes unsymmetrical in the manifestations of the third. If he will let me venture to improve his hypothesis, I would suggest that those of which he speaks as the residues of the secondary period are only such as are formed in the period of irregularity which follows the secondary and precedes the tertiary : a period of irregularity in which we should expect that, after the common manner of all blood-diseases, symmetry would cease to be observed, or would, at least, be gradually less and less distinctly observed. In that case I can fully adopt his hypothesis of the deposits which reappear in the tertiary period being produced in the secondary.

But as to whether the tertiary symptoms may be regarded as not due to blood-disease because they are not symmetrical, we must look to further characteristics for decision ; and I would venture to believe still that syphilis, so long as it exists in any manifestation at all, is a blood-disease. It may be supposed that the secondary deposits, as the results of blood-disease, may be renewed into disease by any external cause ; but this would hardly explain several features of tertiary syphilis. First of all, we can scarcely understand local diseases to be entirely of spontaneous origin ; yet the outbreak of a tertiary syphilitic attack is to all appearance as spontaneous as anything that we see in disease. Then, in local diseases, we commonly suppose that the evidence of disease will bear some kind of proportion to the force with which it has been produced ; and yet certainly tertiary syphilis in great severity may be produced or made manifest by an external force very small in amount. Then, again, most of the tertiary syphilitic symptoms have their distinct or, as they are commonly called, specific, forms ; manners of nutrition, methods of disease, by which they are sufficiently distinguished in our diagnosis from anything that could fairly be called common disease. We can usually distinguish a syphilitic ulcer of the tertiary period from any ordinary ulcer. Very commonly we can distinguish a syphilitic joint from a rheumatic, or a gouty, or a simply inflamed joint. Very commonly, too, we can distinguish a syphilitic periostitis from a common one.

Then, lastly, as another sign of specific character, and therefore, so far, probably of persistent blood-disease, we can discern that, in certain cases, the tertiary syphilitic disease requires specific treatment. I entirely agree with the ingenious expression of Mr. Jonathan Hutchinson that it may be local treatment which we address to a disease, even when we put the remedy into the blood ; that we only—if I may so say—use the blood that it may carry the remedy into the substance of the diseased part ; and yet it is plain that there are certain tertiary syphilitic diseases accessible to external treatment, as well as those which would be considered internal, but not curable by external treatment. The annular and crescentic ulcers of the tertiary and syphilitic periods are, so far as I know, not curable by any external treatment. We may bring iodide of potassium, or mercury, or any other treatment, to their surface, and give it every opportunity of reaching their very substance, but we do not thereby cure them ; we need for the cure of

them, as much as for primary syphilis, either iodide of potassium for their temporary cure, or mercury for their final cure.

Therefore, out of these facts, I should answer, so far as I can, Mr. Hutchinson's first question, by holding that, to the last, so long as there can be any evidence of its existence, syphilis must be regarded as a blood-disease.

I come, next, to that to which I have already adverted, the hypothesis—a most ingenious one, and one in which I am nearly sure there is some real value—namely, that the gummata of the tertiary stage result from renewed growth in deposits left over from the exanthem stage; that, if I may express it otherwise, the exanthem stage of syphilis leaves behind it certain residues in which the disease may be renewed by any casual external conditions. I cannot cite instances of this from my own observation in syphilis; but instances may be cited in general pathology and from common diseases which, at least, will go far to render it not improbable that this may be a true hypothesis, even although there may not be discernible, in the parts which become seats of the tertiary syphilitic disease, any change of structure whatever. I published, some years ago, an instance which occurred in my own person, to show how a residue of disease may be left unobserved, and yet ready for outbreak upon any fit occasion. I had applied leeches to my wrist for a sprain of it, and four or five years elapsed with no trouble whatever in the scars, except that I could count them on my wrist. At the end of five years, when the scars might have been deemed to be as sound as any other texture of the body, I was for the first time exposed to the heat and other conditions of travelling in Italy, and then at once every scar inflamed and ulcerated; and so it occurred for every year in succession when I was exposed to the same influences; every scar and every leech-bite inflamed and suppurated again, and it took four years of similar exposure to those causes of disease before the scars settled down into a condition in which they could resist these influences.

And, if I may tell another personal experience, it will bear with some force on the question of the necessity of studying residues of disease. Four years ago I had an attack of acute erysipelas following dissection-poison. It affected only my left arm and part of my left side, the right arm being wholly unoccupied by it. My left arm recovered, as I supposed, without a blemish in it. For years I went on without the smallest consciousness that there was anything

in that arm which could be affected by any force which would not equally affect the other arm. This winter—I hardly know why—I took cold baths instead of warm ones, and, on every application of cold water to the arm, in which, let me repeat, I am wholly unconscious of any difference from the right arm, there ensues lividity; there is a marked difference between the left arm and the right; such a difference as would spoil the symmetry of any constitutional disease with which I may be affected; such a difference as might be marked by a greater tendency to production of disease in that limb than in the other; a clear residue, not discernible by any other means than those applications of cold; a residue very similar to that which may occur after any specific disease to which any man may be exposed. I am disposed to parody the well-known expression, “Let no man be accounted happy until he is dead,” by “Let no man be accounted healthy until he is well examined after death.”

There is only one point more in Mr. Jonathan Hutchinson’s paper to which I will refer, and that is one in which I feel bound for the first time, not materially, yet in a measure, to differ from him. He enters upon this question—one of the greatest interest—in reference to the relation which syphilis bears to other diseases, and asks, Does syphilis in any way predispose to scrofula or tuberculosis? In answer to this, I should agree with him absolutely that it does not so predispose in any other sense or degree than any other fever does. Syphilis, by long-continued illness and deterioration of health, may let in scrofula or tuberculosis; but so may rheumatism, so may gout, so may typhoid, so may any other disease which, to use the plainest words, in any degree impairs the health of the person disposed by inheritance to scrofula or tuberculosis. Then he speaks of the relations of lupus to syphilis. And there, too, I agree with him wholly that lupus is in no proper sense a syphilitic disease. It is definitely a disease of tuberculosis. I doubt whether any persons the subjects of lupus can be found in whom there might not be traced a well-marked inheritance of tuberculosis; and, if there be a syphilitic lupus, I should hold that it can be found only in persons who become syphilitic after having inherited the constitution out of which lupus is likely to occur; but, when Mr. Hutchinson says that, as a rule, we do not observe any modification of one by the other—referring there to syphilis as it may occur in strumous or tuberculous people, in

those who suffer from gout or psoriasis, and those who have extreme enfeeblement of circulation, I cannot but differ from him. I think that one of the things we have most to study, both in the pathology and treatment of syphilis, is the modifications which it undergoes in persons of different constitutions in whom it may be inserted.

I should hold this even in regard to the primary syphilitic sore. Agreeing with Mr. Hutchinson that there is but one syphilitic virus, I yet cannot but feel that we have to explain the singular variety of forms in which that virus, or the pus in which it is contained, produces its effect in different persons. We may suppose that in some persons the pus is inserted without the virus, and in others not; but it would be an extreme difficulty to tell any means by which the virus can be separated from the pus. I think we should look for facts which would indicate that, out of the constitution of each person into whom the syphilitic virus is inserted, there may come a different—not essentially different—but a modified, result. The facts, indeed, are like those which may sometimes be discerned even now in the insertion of the vaccine virus, to which some are less and others more subject, and some absolutely indifferent, or such affections as we have on record from the old histories of inoculation, where the disease produced was the same in kind, but very far from the same in degree, in all persons in whom it was inserted. So, too, in the secondary period, I think we may see, however obscurely, that there are differences due to the differences of the constitutions of the persons in whom the secondary disease occurs: but much more in tertiary syphilis, in that period in which the syphilitic virus seems to be losing its power or intensity, and the natural constitution of the person comes to have more and more share in the general product of the disease.

I would not venture to call the disease that may occur in a scrofulous person, become syphilitic, a hybrid one: and yet, perhaps, the term is not altogether wrong; but, at least, I would call it a mixed disease, and hold that syphilis inserted in a scrofulous person will, in its tertiary period, produce signs which it may be very hard to distinguish from scrofula—signs in which the characters of scrofula and of syphilis are mingled, and—which is very important—which require that the treatment of scrofula should be combined with the treatment of syphilis, in order to produce a fully successful result. So, too, in gouty persons, and in the rheumatic

and the neurotic, I think we can discern mingled characters of syphilitic disease with the diseases to which their constitutions make them liable; and I think I can be sure, too, that all these several persons require for their treatment that we should have in view not only the specific disease but the constitutional disease with which it is mingled; so that the treatments for the natural constitution and the acquired constitution may be combined.

And this is quite confirmed by what we observe in the sequels of other fevers; for clearly, when we watch the sequels of typhoid fever, we have to explain why one person has phlebitis, another necrosis, and another suppurating disease of a rib; and I believe that those differences may be explained in discerning different constitutional or even different local peculiarities in the persons affected. I am acquainted with an instance in which five members of one family, having suffered from typhoid, have all had affections of the nervous system as its sequels; and the worst case I have yet seen of rural phlebitis following typhoid is the case of a gentleman in whose family I know of four other persons all suffering from disease of the veins of the lower extremities. These, it may be said, are instances in which the sequel of the disease falls only on a part which may be considered weak; nevertheless, that which is true of a part would also probably be true of the constitution; and, at least, we have to keep in mind that, among the many diseases which we have to study, few are so variable in their phenomena, taking them altogether, as syphilis.

Now, we have cast aside—at least, I have, with Mr. Jonathan Hutchinson—the belief in the multiplicity of poisons. But, if there be one poison only, how is it that that one poison produces so many various effects. We cannot ascribe it to the various external conditions in which the patients are placed; neither can we reasonably ascribe it to the variety of treatments; for the treatment of syphilis, as now carried on, is, for the most part, very similar in all cases. It seems to me that the only thing to which we can fairly look is to discern the differences produced in the one disease by the variety of persons in whom it is inserted—the variety of soils, if I may so speak, in which it has to develop and to grow.

These are all the observations that I would make in regard to Mr. Jonathan Hutchinson's paper; but I would not sit down without expressing my admiration of the paper as a whole, and of the thorough, profound view which Mr. Hutchinson has taken of

the whole subject. I venture to make myself proud in calling him one of my pupils. Not my pupil in syphilis: with regard to which I say nothing, but that, at least, I did not interfere with his studies while he pursued them with myself in the out-patients' room of St. Bartholomew's Hospital, where I saw nothing but confusion, and he saw light coming. And I venture to say, if any of us think that in this instance Mr. Hutchinson is looking into outer darkness we shall be very unwise if we neglect his signals when he says that light is come.

Dr. WILKS.—Having been asked to say a word or two on the present occasion, I rise to do so, having always felt an intense interest in the subject of syphilis; I am afraid, however, that I can throw very little fresh light on the matter, my views being much in accord with the general doctrines laid down by Mr. Jonathan Hutchinson. Permit me, in the first place, a little ebullition of feeling in this matter. When I remember it was only a few short years ago that the subject of syphilis was discussed in this Society in the crudest possible manner, and when I think of the philosophical address which we have had presented to us, I cannot but feel that more has been learned of the nature of syphilis within the last twenty years than was known during the four preceding centuries; and I cannot but think that the Pathological Society of London has had a great share in this advancement. The younger members of the Society are, perhaps, scarcely aware how we had to fight this question of visceral syphilis in the Society. Being much interested in the matter, and thinking it over this morning, I took the opportunity of referring to a medical journal to see how very recent the modern doctrines were. In a so-called review of my 'Lectures on Pathological Anatomy,' extending to about twelve lines, one of the leading journals said: "We cannot but think that he errs in laying down as law what is considered by most authors still *sub judice*. For example, syphilitic fibroid deposits are mentioned as of frequent occurrence and of unquestioned character. Now, most of those who have paid attention to the subject regard the proof of this statement of Dr. Wilks as anything but convincing." Taking the intense interest that I do in the advancement of my profession, if I were asked in what direction progress has been made in it, I should be vain and conceited enough to say in the direction of pathology; and if I were asked in what par-

ticular department, I might mention that of syphilis, and I should point to this Society as having had a large share in the work. The younger members have brought, night after night, specimens in the despised soup-plates, endeavouring to improve our knowledge, until we have seen the crowning edifice in the address of Mr. Hutchinson. The interest I feel in this matter, and which I have no doubt Mr. Jonathan Hutchinson feels, is not on account of the wide-spread prevalence of the disease, or of any of its social relations, but from its great pathological importance. There is not a disease like it in our nosology. It is for this reason that Mr. Hutchinson has taken so much interest in the subject, and also his great master, John Hunter. In all our pathological researches, we are endeavouring to find out the why and wherefore of disease. Here is a wonderful example for us. A healthy man has a small amount of virus introduced into him, perhaps an infinitesimal amount, and the whole of his nature is changed; he breaks out into a rash over the whole surface of his body, and all his viscera are affected. For years and years afterwards any morbid process that may take place is entirely altered in its character. The man brings into the world a number of puny, ill-formed children, and the result is, perhaps, seen even in the next generation. There is no example like it in pathology. It is a large experiment made for us, and that is why so many of us take an interest in this affection.

I quite agree with Mr. Hutchinson as to this disease being a counterpart of the other febrile diseases. I have always regarded syphilis as a disease *per se* attended by its sequelæ. The physician looks upon it as a constitutional disease, and to him the words primary, secondary, tertiary, and quaternary have no meaning. Either a man has syphilis or he has not; he either has a poison in him producing all these peculiar morbid products, or he has not; and I believe that all these visceral changes that we observe are due to the true syphilitic process, and take place, I have no doubt, at the same period of time. When we examine the body of a man who has had syphilis we may find, no doubt, hard fibroid nodules in his body; but one knows, as a fact, that these nodules may have been felt in the liver, for years and years, whilst he was in a state of comparative health. Therefore I have always regarded them as a sort of *débris* of the true syphilitic deposit. That they may undergo further change, or grow, as Sir James Paget has said, I

feel little doubt, and also because we see similar inert masses increase in size in the body. But, of course, there must be some morbid process previous to their formation. We have evidence of it in those very cases where fibroid deposits have been found; we know that the patient during the exanthematous eruption really had enlargement of the liver, with affection of other organs; therefore we are bound to believe that there was a stage previous to that of the nodules, and we want to know exactly what the condition of his organs was at that period. We require more information on the subject, and we want the members of the Society to bring us specimens, if they can, of these earlier changes that take place in the viscera. That they are very marked there can be no doubt. I saw a case of this kind the other day. The patient had enlarged testes and an enlarged liver; there was dulness at the upper part of the lung, and, besides that, there was a node on the tibia and a node on the forehead. Under treatment the liver went up, the testes got small, and the lungs returned to a healthy condition; therefore there can be no doubt that similar processes in all these organs must have been going on at the same time. I have had the opportunity of examining the liver in syphilitic children, and have found what was apparently a healthy-looking liver somewhat enlarged, but very hard, and on examining it by the microscope it contained merely a little fibro-cellular tissue with a healthy structure remaining; therefore there could be no doubt that the former was absorbable material. Whether the same occurs in the adult I do not know. One specimen was brought to the Society lately. The man had the most extreme form of syphilis that I have ever seen. The number of gummata in the body was extraordinary—in the chest, the head, and elsewhere. He had a very enlarged liver, but the deposit was very like miliary tubercle, and very much resembled the specimen that was brought here by Dr. Weber a year or two ago. The possibility of meeting with these early conditions is so exceptional that we really are much in want of information as to the condition of the organs while the syphilitic poison remains in the body. Then, again, that some of the affections of the organs are acute one knows from observation. I have seen three or four cases of jaundice occur in constitutional syphilis, besides a case of acute atrophy of the liver, and I have read of one or two more cases of the same kind. The old impression was that the deposits I have spoken of were subsequent to the secondary

ones that are seen on the body, such as the rashes and sore throat. For my part I think they are cotemporary; that they are an evidence of the true syphilitic poison working in the body, and yet I must admit that they are not frequently seen. It is rather the exception to meet with those internal affections in an ordinary case of syphilis. My impression is (and to this I would call Mr. Hutchinson's special attention) that they stand in something like an inverse ratio to what are called the ordinary syphilitic symptoms. I might illustrate what I mean by the case of scarlatina, because I quite agree with him that syphilis is a true fever; having its incubation and its course, like other specific diseases. The old idea was that kidney-disease was a consequence or a sequel of scarlet fever, but there is now another opinion—and that I hold—that the scarlet fever poison is manifested on the skin, on the throat, and on the kidney; but in exceptional cases the force of the poison may be directed to the throat, when we have a scarlatina anginosa; or to the kidneys, when we have nephritis. I do not, therefore, look at nephritis as a consequence, but as a part of the scarlatina disease. I think the same of syphilis, that where it does not exhibit itself outwardly the internal organs are more likely to be attacked, and I hold the opinion for this reason. When I was commencing to work at this subject, nearly twenty years ago, my seniors, being very sceptical with regard to these syphilitic deposits, asked me for proof, and to my great annoyance the proof was found wanting. They used to look at the dead body for a scar in the groin, and then it was afterwards found that a scar in the groin did not mean syphilis. In going into the history of the case, I found that the ordinary symptoms of syphilis were wanting, much to my annoyance. Then I came to the opinion that in these cases they were really very often absent, and that opinion has strengthened with me ever since; and I may ask physicians here, or those who see cases of internal syphilis, if it is not true that they find a great difficulty in getting a distinct history of what are called ordinary secondary symptoms. The patient comes to you with epilepsy, or an enlarged liver; it is clearly syphilis. He will own to the disease, and will tell you about the sore. Perhaps he has a bad throat, or a little rash, but the whole history of secondaries is doubtful in the extreme, and yet in such a case we find internal disease. The opinion has therefore strengthened with me that there is a kind of inverse ratio between the ordinary symptoms of

syphilis, those that are generally described by surgeons, and these internal affections; and I think that may account for the different opinions that are held by many. Some say that they are due to the same syphilitic process which is going on in the body during the so-called secondary or constitutional stage, whilst others say that they are altogether subsequent to it. My opinion is that they are truly syphilitic, and yet they occur in exceptional cases. It is especially to direct Mr. Jonathan Hutchinson's attention to this that I have risen. I have no doubt that his attention has been already drawn to the circumstance I allude to, and I shall want a very confident opinion from him one way or the other about it. I will mention another reason for believing that the internal affections arise in the early period of syphilis, although their occurrence is exceptional. I have said that in these cases of internal visceral disease the testis is often affected, and just as sarcocele becomes smaller under treatment, so is absorption going on in the other organs, so that you may take the condition of the testis as a kind of gauge of the curative process. Now, if these internal affections, combined with that of the testis, are very common in the ordinary exanthematous stage, the latter ought to be known to surgeons. I am not aware what they may say now, but when I was more interested in the subject, I asked all my friends the syphilists, those who had large experience in the matter, "What do you know about orchitis in syphilis?" Many of them said, "I know nothing about it; you mean gonorrhœal orchitis?" "No," I said, "I mean syphilitic." "I do not know anything about it," was the answer I received from the most experienced surgeons. I think it is very clear from this that if the sarcocele does not ordinarily occur in the exanthematous stage, neither do the other visceral inflammations in all probability take place, and yet as both the visceral affections are sometimes met with during the early period of the disease, I consider that when they are present they take the place of the more manifest external ones.

I am very glad to hear that Mr. Hutchinson speaks of the pustular rash as occurring during the virulence of the disease. We very much want to know if we can measure the progress of the disease by the rashes. The old idea was that we had exanthems, papular and scaly rashes, at the early period, and afterwards the moist rashes; and I suppose this is more or less true. That we may get pustular rashes during the height of the disorder, I have

no doubt—during the period of the gummata. Mr. Hutchinson also speaks of this gummatous matter never suppurating. With that I entirely agree. I suppose every surgeon knows that a node on the forehead may be met with, which will soften, and fluctuate; it feels like matter, but it is all absorbed under treatment, or if it be opened it will be found that the matter is not purulent. I had a case the other day which bore upon that; it was an exceptional case proving the rule. A man in the hospital had what I thought was an abscess in the liver; that is, there was a large collection of fluid in the organ with marked fluctuation, and yet I had no doubt that the man had syphilis: then the question arose whether the syphilitic matter had suppurated. I said I had never seen such a case, although this seemed like one. When we opened it the fluid was not purulent, but broken-up syphilitic matter. It was an exceptional case, which tended to prove the rule that this syphilitic matter disintegrates, but does not tend to suppuration. I suppose Mr. Hutchinson holds the idea that this disease may run on for many years, that it may be arrested, but still it pursues a definite course. I should like to hear a little more about that, and whether we can tell exactly at what stage the disease has arrived. I apprehend that it may go on to a certain point and then stop, and that stage may last, as Sir James Paget has said, over ten, fifteen, or twenty years. There used to be a notion that a man might have syphilis, and then have a relapse; go backwards, again forwards, and so on. Now, if it be a distinct disease running its course, this cannot be. It may be arrested in the middle of its course for some reason or other; Mr. Hutchinson suggests by treatment—and perhaps this may be true; and then it breaks out afresh. We want to know, then, if by the character of the rash we can say exactly at what stage the disease had arrived. The old notion was that we had in the early period dry rashes, or scaly rashes, and afterwards pustular ones; but those who write on skin-diseases, especially the French, speak of scaly rashes at the very later periods. Perhaps when they say scaly they only allude to the psoriasis palmaris, which one sees at some of the later stages of syphilis. When I was on the Government Syphilitic Commission I questioned many of the experienced surgeons as to points of this kind, and I did not get any information from them. We heard that the disease might last fifteen or twenty years, when the man was supposed to be well, and then it would break out again. I

said, "Do you mean that the patient who has had rupia or ecthyma will come to you years afterwards with a scaly rash?" They did not know. We want more information as to the defined character of the stages, however prolonged they may be.

There is one point which Mr. Hutchinson has not gone into—whether an adult may take the disease at any of these stages, in the same way as a child who has inherited syphilis never has the primary disease, but has it at the second stage. There is a notion of this kind abroad—I believe among the French; and one distinguished man speaks of two forms of syphilis, or as acquired from a primary or secondary sore. It is an important question to surgeons in reference to contagion, whether they may be so or not. At one period it was believed that a sore in a man could only be obtained from a sore in a woman. Now we know that secondary disease is contagious; but if it be so, it has been thought that poison from the secondary disease is different from the other, or rather that, in the person who is infected, it may begin at the second period. I do not suppose that Mr. Hutchinson holds that opinion. I have no doubt myself that the disease is propagated mostly by women who simply have the secondary disease, and the man gets what may be called a primary sore. If this be true, there is an end of the question and the syphilitic poison is the same at whatever stage of its manifestation on the body it is propagated; but this is not a universal opinion held on that last condition. Only four years ago, a gentleman said here that it was perfectly useless to examine a woman under the present Act of Parliament, because the disease was propagated to an enormous extent in London, where the women were said to be healthy, clearly showing to him that there could be small ulcers existing in the genital organs without their being discovered. We have no reason, however, to suppose this because we know that secondary disease can be propagated.

Mr. Hutchinson made a remark, to which, perhaps, I have no business to allude, that is, about primary chancre having a secondary deposit in it. The subject interested me as I have seen one or two cases of it, and I rather thought that Mr. Lec's account of it explained the matter somewhat better, because it has its analogies in smallpox. I maintain that when a man has indurated chancre he has not syphilis. When Ricord first propounded that it was of no use to remove a chancre, because the poison was already working in the system, and that induration was the first secondary

symptom ; I accepted his statement in a certain sense as true, but if you regard the disease as a fever and as an exanthem, it is not true, for the indurating process is merely the period of incubation : as in the analogous case of inoculation by smallpox. If I were to show any one here a patient with a smallpox pustule upon him, he would say the man has smallpox. It does not follow. If he caught smallpox in the ordinary way, and then it broke out in pustules, it would be so ; but if I inoculated a person with smallpox the vesicle would form and grow for a week : during which time the patient would not be ill, although he would have a pustule ; and so it would be until the end of the period, when the incubation was over ; then he would break out in a general eruption, and fresh vesicles might occur in the very spot of inoculation. That used to happen in old times, when inoculation took place, and I apprehend it is much the same in syphilis. When a person is inoculated with the virus of syphilis, a chancre is formed ; there is induration, but I do not see that that can be called the first secondary symptom ; a month must elapse, when the whole system is infected and the eruption breaks out. I think the two diseases are perfectly analogous in this respect, and so a secondary deposit may occur in the seat where the original virus was implanted.

I have a note in reference to the course of the disease in relation to those remarkable experiments of Professor Boeck, which I do not think have received the attention they deserved in this country. Professor Boeck maintained that syphilis was a specific disease, that it was arrested in its course by treatment, but could not be cured until it had come to its natural end, and if it did not end you must make it end by putting in fresh virus ; so he kept inoculating the patient until the disease was complete. That was not approved of here ; in fact we thought little of it, because we said he did not inoculate syphilis, but merely took the matter from the soft sore, as he did not distinguish between the two. Professor Boeck said this question was unimportant and afterwards declared that tartar emetic might do as well. It seemed so ; for he produced a number of pustules on the surface, and if you can believe in the genuineness of his cases, several hundreds of which he published, the patients did get well. It seemed that the poison was in some way eliminated by his process and could not be affected any more, just as skin-diseases will cease in a long affected part, but spread on a new surface.

I will now say a word or two in reference to the question of sero-

fula. I am rather inclined to echo what Mr. Hutchinson, rather than what Sir James Paget said on this matter. I believe I can tell a child who has had syphilis from one who has had scrofula. The appearance to me is very different. If there be a difficulty, and we apply treatment, the difference is soon seen. I can recall cases of so-called scrofula, occurring many years ago, that we did not cure, and which now we readily relieve because we give them another name, believing they are due to a different disease. I remember that, when Mr. Hutchinson brought out his work on hereditary syphilis, there was a little boy in Guy's Hospital who had a large ulcer in his throat, which was at first thought to be cancer, and then to be scrofula. He had port wine and quinine and cod-liver oil ordered him, and he was in the hospital for months. Having seen Mr. Hutchinson's book, I said, "The boy has syphilis." There were the marked teeth, nose, and head, the exact counterpart of that which Mr. Hutchinson had described. We gave the iodide, and rubbed in a little mercurial ointment under the arm, and we could almost see the ulcer heal. In a fortnight it was nearly well. As regards treatment, therefore, the two things are thoroughly different. Then the general configuration of the person is different. We had to discuss this question at the venereal commission. We had some first-rate dentists there who were sceptical with regard to Mr. Hutchinson's view about teeth, and said they were nothing more than scrofulous teeth. I think I know what scrofulous teeth are, and I know a scrofulous head and a scrofulous jaw; the upper jaw is narrow and the teeth are all crowded together. In many of these syphilitic children you see that the teeth would have been absolutely perfect, if they had not been affected in this way, but they have not the appearance or character of scrofula. Thus in every particular I am rather in accord with Mr. Hutchinson, that the two forms of cachexia are absolutely distinct.

Dr. FAGGE.—It has appeared to me that there is one point of view from which the subject of the relation between syphilis and its more remote effects may be regarded, which Mr. Hutchinson has scarcely approached, and which may have important bearings. I refer to the influence of syphilis in producing lardaceous changes. I think that in the *post-mortem* room, if we find any of the viscera lardaceous, we may be sure it must have been produced by one of two causes: either the patient has had, for some time,

chronic suppuration, or he has suffered from syphilis.¹ If, in a case in which the organs are lardaceous, we look over the body, and fail to find any source of suppuration, I believe that we may be certain that the patient has had syphilis. I know it was at one time supposed that syphilis produced lardaceous disease only indirectly, and through the agency of suppuration; but experience shows that such is not the case. We find lardaceous disease in syphilitic patients when there is no reason to suppose that there has been suppuration. I do not know how this effect is produced, but I think we may say that it is difficult to conceive how advancing science can hereafter show any direct community of operation between these two distinct causes of lardaceous disease, and that we are almost justified in saying that it can only be by some general influence upon the health. It is true that lardaceous affections are the result of a change in the chemical constituents of the tissues, and so differ altogether from affections in which the pathological process is of the nature of a new growth; but if we find that syphilis is capable of inducing such a change, surely we are justified in believing that it may affect the general health, depressing it, and rendering it more susceptible to other influences which give rise to disease. It is from the relation of syphilis to lardaceous affections that I should be disposed to approach the relation of syphilis to tubercle. I know, of course, that Dr. Wilks and others believe that syphilis may produce in the lungs changes which are quite easily recognisable by auscultation, and which disappear when the patient gets better of the syphilis. There may be diffused changes in the lungs, of a syphilitic character, which get well. I cannot say that I have been convinced that that is really the case. I have seen many syphilitic patients die of phthisis. It never appeared to me that any considerable change in the lungs was really syphilitic. Even with regard to small nodules in the lungs mistakes may be made. Last year a man died with gummata in the internal organs. There were some masses in the lungs, which I at first thought to be gummata. But afterwards, when the specimen had been hardened, I found that the whole of these masses really consisted of patches of caseous pneumonia: in every part of them the air-cells are still plainly visible, being filled with ordinary inflammatory products. So I

¹ Detailed evidence in support of this proposition was brought forward by Dr. Fagge at a subsequent meeting of the Society, and will be found at p. 324 of the present volume of the 'Transactions.'

should not admit as gummata in the lungs any masses, the nature of which had not been carefully tested by microscopic examination. It seems to me that the fatal lung disease of syphilitic patients is not a definitely syphilitic affection of the lung itself, but an ordinary phthisis due to a remote influence upon the general health, comparable with that which causes lardaceous changes. I have formed this opinion in spite of myself. Having begun to study syphilis with regard to its effects on the skin, I have always held strongly an opposite view with regard to syphilitic eruptions, that to the last they are specific, and distinct, and capable of being recognised as different from other cutaneous affections. For instance, I have seen a patient who had from childhood been affected with ordinary psoriasis, and who came under treatment for syphilis; and, at the end of the case, after the administration of mercury and iodide of potassium, it has been easy to recognise the original patches of psoriasis, which were entirely uninfluenced either by the disease or by the treatment. So I am not disposed, generally, to mix up syphilitic affections with non-syphilitic affections; yet I cannot help feeling that, if syphilis can produce lardaceous changes, so it may produce phthisis, and, possibly, other diseases likewise.

MR. BERKELEY HILL.—I venture to solicit permission to occupy the time of the Society for a few minutes with some remarks upon some of the conclusions set forth in Mr. Hutchinson's introduction to our debate. Many of the comments I should desire to put forward have been far better stated in the eloquent and exhausting address of Sir James Paget. But let me express my admiration at the manner in which Mr. Hutchinson has performed his task. The description of syphilis which we listened to the other evening is probably the most complete that has ever been compressed into fifty minutes' reading. His digest, reducing to what order is already known in most departments of the natural history of syphilis, will form the framework to which future investigators will add the yet undiscovered details. To have put forth so authoritative and clear a summary of what is known of syphilis, reflects great credit upon our Society. At present it is rather by examining the conclusions Mr. Hutchinson has drawn that we may most usefully aid the object in view.

In the first place I would beg to demur, so far as I understand Mr. Hutchinson, to supposing that "*dualism is dead*," and that the opinion generally prevalent holds the soft chancre to be a conse-

quence of the inflammatory products of syphilis. So far from this being the case, several recent text-books on general surgery or on venereal diseases state that the soft sore is distinct in nature and origin from syphilis.

Without occupying your time with all the names I might quote, I may mention, in English, Erichsen's 'Science and Art of Surgery,' and Bumstead's 'Venereal Diseases;' in French, Fournier's exhaustive article, "Le Chancre," in Jaccoud's 'New Dictionary of Medicine and Surgery;' in German, Zeissl's 'Syphilis,' and v. Baumler's article "Syphilis" in Ziemssen's Cyclopaedia. At present it would, I think, be more correct to say that while much diversity of opinion still exists, the doctrine first suggested by Ricord and established by his pupils that soft sore and syphilis are distinct in origin and nature, is gradually spreading throughout the profession. This at least is the view taken by Billroth in his 'Surgical Pathology.'

A more important matter is the view that syphilis is a *specific* fever, to be classified on that ground with smallpox or scarlet fever. This seems to me misleading, and likely to produce mischief in the prevention and treatment of the disease. Mr. Hutchinson tells us that better acquaintance with the acute eruptive fevers shows that they vary quite as much as does syphilis in length of stage, in degree of severity, and in the occasional omission of some of their phenomena. Let them vary as they may, any one of the recognised specific fevers taken as a whole is, I still think, strikingly different from not only the ordinary course of syphilis, but from any extraordinary course of that disease.

By taking advantage of these irregularities in the course of acute fevers, by abruptly limiting the duration of the secondary period, and by pushing into the back ground the later phenomena as merely local affections, Mr. Hutchinson produces a picture that at first sight bears a resemblance to the eruptive fevers. This resemblance will nevertheless not bear close scrutiny. If our discussion establishes syphilis among the eruptive fevers, it must I think be by the contribution of new facts not yet laid before us.

An important point with Mr. Hutchinson is to draw a great distinction between the so-called secondary and tertiary symptoms of syphilis, that the former may be considered effects of general blood-poisoning, and the latter only local remanets of the exhausted infection.

It is unnecessary to encumber the discussion with additional

examples of the acknowledged characters of syphilis, already so well drawn by Mr. Hutchinson, for a different appreciation of the facts he enumerates form an ample structure to suffice to establish and support the theory which sets no absolute or definable limit between the several periods of the disease—the primary, the secondary, the latent and tertiary stages.

I contend that at no time can a lesion of syphilis be termed a mere local affection, still less that the blood is ever free from participation in the disease. On the contrary, from the very first appearance of induration at the point of contagion to the latest syphilitic phenomenon, we have to deal with an essence that influences all the tissues.

Certain characters Mr. Hutchinson has grouped together as common to all periods of syphilis, to that which he would have us call the season of local affection, the so-called tertiary period, as well as to the earlier stages when the blood and all other tissues are affected.

These characters I shall claim as sufficient evidence of the essentially chronic course of syphilis that renders it distinct from the recognised fevers. Mr. Hutchinson selects others on which he relies to prove that a radical change takes place in the nature of the disease when the symptoms become apparently isolated rather than general, and follow each other at long rather than at short intervals.

He tells us that *symmetry* is a characteristic while the blood is infected; both sides of the body are attacked and several of the regions which can be searched by the eye are simultaneously the seat of morbid change.

Acknowledging that little is yet known concerning the affections of the viscera and deeper structures at this early period, Mr. Hutchinson suggests that they usually occur simultaneously with the early eruption on the skin, but spontaneously subside after a brief existence; in this respect they would correspond to the ephemeral symptoms of the surface. It may be that there is some truth in this conjecture, for it is no more than conjecture; nevertheless, I would remind him that recent contributions to our knowledge of early visceral syphilis indicate these affections to be similar or identical with the later so-called tertiary affections with which we have been long acquainted. But be this as it may, symmetry, Mr. Hutchinson confesses, is a distinction of abundance rather than of character; and if he recollects how gradually the

general widespread rashes that first appear are replaced by succeeding rashes each more limited than its predecessor, we may be pardoned for not seeing in the cessation of symmetry a very clear demarcation between the early and later periods of syphilis.

Another distinction is the spontaneous resolution of new growths that is constantly witnessed in the early stages. Though very usual then, he admits it is not peculiar to the secondary period, for a little further on in his description of tertiary products, Mr. Hutchinson remarks that their spontaneous resolution often occurs.

Indeed, I think he will acknowledge that the natural tendency of every syphilitic morbid process is to cease without permanent loss of tissue; for, though ulceration usually attends the growth and waning of tertiary affections, such a complication is occasionally the accompaniment of early syphilides.

In addition to symmetry and spontaneous resolution there is the cessation of the contagious condition which Mr. Hutchinson believes to happen when the general rashes disappear and the latent period arrives. I shall endeavour to show that termination of the contagious condition is also gradual, and not a phenomenon suddenly produced in any case.

The facts which lead me to hold syphilis to be a chronic malady, different in nature from a recognised fever, are:

1. The histological identity of the morbid processes at all periods.
2. The frequent appearance soon after contagion of those affections which are commonly delayed to a late period.

This precocious development of gummata shows the identity of the disease in the early and later stages, and probably also the intensity of the virus, for early gummata and other tertiary affections are most frequently observed in severe cases of inherited syphilis where the parent is recently infected. Some of the most typical of visceral gummata are seen in stillborn children.

3. Certain antidotes can subdue every process of syphilis even to preventing contagion, and these antidotes are effective at all periods, both early and late, of the disease.

4. But the most important evidence for my purpose is the long duration and gradual extinction of contagion, in a manner peculiar to syphilis and quite distinct from the contagion of acute eruptive fevers.

Syphilitic contagion is not volatile nor does it appear to be soluble in fluids, but to require an organized cell for its conveyance, the blood-corpuscle, the spermatic cell, and the ovum, being those against

which the best evidence of guilt in this respect has been collected. Propagation takes place in two ways, by contact and by hereditary transmission.

The former, or propagation by contact, is most frequent during the secondary period, but ceases in most cases when the latent period is reached, that is, in two years or thereabouts after infection. But it is important to note that it does not cease abruptly, as should be the case if syphilis were analogous to the acute eruptive fevers, but may reappear at intervals during relapses of the disease before it finally dies out.

The second mode, propagation by inheritance, very active during the early stages of the disease, retains its power through the latent and tertiary periods, requiring in certain individuals many years to pass away before it becomes thoroughly inert.

Again, a very distinctive point, during this period the virus is not changed in nature, but only in potency by lapse of time. This peculiarity is shown in a striking fashion. Syphilis, whatever phase it presents in the parent, always developes in the infant as early general disease. Tertiary affections, indeed, notin frequently develop in the infant with the earliest cutaneous rashes, when the parent has not been long infected.

Yet another point which demonstrates also the continuity of the disease, namely, it is immaterial whether a syphilitic infant have been infected by a secondarily or a tertiarily affected parent; the secretions of such a child communicate the ordinary form of syphilis to his attendants as readily as do those of an adult in whom the early exanthems and erosions are in full career. These characters of the contagion are insuperable to any allocation of syphilis with acute fevers.

This curiously slow exhaustion of contagion has been fully described and put beyond question by Dr. Kassowitz, whose remarkable memoir has recently been noticed in the 'British Medical Journal.' When the disease of the parent is not treated, or only insufficiently so, its course is commonly thus. While infection is recent the first foetus is born dead prematurely, the succeeding foetus may also be prematurely born, or if born at term dies in a few hours. The next child is attacked soon after birth and succumbs in a few weeks. When years have followed the parent's infection the child, apparently healthy at birth, shows at the end of the fourth week symptoms of the disorder. In

succeeding children the disease may delay its appearance as late as to the end of the third month, but is never postponed beyond that time. Kassowitz has demonstrated that three years must be expected to elapse after infection before a viable child can be born, and many more years before the influence is finally exhausted. When the parent is subjected to careful mercurial treatment a healthy child may be born long before the disease is finally quelled.

The exhaustion of the contagious influence is so regular and gradual that the fate of the fœtus can be foretold when the age of the infection in the parent is known, and *vice versâ* the duration of disease of the parent can be calculated by the symptoms of disease in the infant.

Here I should like to suggest the possibility of a similar effect in acquired syphilis to this enfeeblement in the hereditary disease, but for which I have insufficient evidence to propose to establish as a character of syphilis. It is this—sometimes the course of the disease in a patient is mild when he has contracted it from a person whose disease is of long standing and approaching the latent period.

On the other hand, in cases of severe course and copious development the patient has been infected from a person with recent syphilis. I have only records of thirteen instances where I was able to compare the infected patient with his source and in whom I had sufficient reason to believe that the supposed source was the real one. Moreover, I believe that conditions existing in the individual himself are powerful over the course the disease shall take. Hence, while bringing forward this fact as a suggestion I refrain from urging it as a law, until ample evidence has been collected.

Dr. Wilks has told us that he long ago suggested that those who suffer from the inner and deeper forms of syphilis have a poor story to tell of the superficial affections of the early stage. This, which is doubtless quite correct, I probably learned from Dr. Wilks, and forgot that I had done so, for I have been in the habit of supposing that I discovered this point myself. I rejoice to be reminded that the existence of this character rests on Dr. Wilks's testimony.

Somewhat the converse has occurred to me as being also the fact; namely, if a man have long-continued palmar or plantar psoriasis, for example, we may pretty confidently assure him that he will not be troubled by the deeper affections of syphilis; that if the disease expend itself mainly and continuously on the skin, he will probably escape the visceral lesions which are so fatal.

The characters which I have recited are, it seems to me, entirely

different from those of the acute fevers. These maladies do not commence with an initial lesion; they have a rapid course abruptly terminated by convalescence, free from relapses, and their volatile contagion does not remain smouldering in the parent to break forth into flame year after year in the child. Lastly, no chemical agent is their antidote.

Most unlike such disorders, syphilis consists of a series of morbid processes all essentially identical and almost repetitions of each other. Proceeding from the imbibition of a virus, copious at first and besetting all the tissues, these processes become fewer as the energy of the poison diminishes, and ultimately disappear altogether unless roused again and again into activity by favourable condition of the poisoned individual.

There is another point in which I desire to say a word. Mr. Hutchinson did not allude to the conditions of the individual which prolong the course or increase the severity of the disease. I mean, for example, the lymphatic temperament or the scrofulous diathesis, or those artificial states of debility produced by alcoholic indulgence, great bodily fatigue, exposure, or insufficient nutriment. Such are commonly held to be powerful factors in determining the severity and possibly the duration of the disease. At the least they must be allowed for when considering what influences determine the prolongation of syphilis into the tertiary period.

With regard to scrofula and tuberculosis Mr. Hutchinson has pointed out that syphilis never merges into them nor have they any ancestry in syphilis. Doubtless much that was once considered scrofula is really syphilitic, and when all that is tubercular and that which is syphilitic is separated from the truly scrofular, there is so little left that I have been inclined to wonder if there is such a diathesis. It will be interesting to hear from Mr. Hutchinson how he distinguishes the three diatheses.

Mr. DE MERIC.—I may be permitted to allude to Mr. Hutchinson's address as one worthy of himself; one which has not disappointed any of us. We know well that anything emanating from him must be eminently philosophical and true. But I am afraid that, like all men of great merit, and men thinking a great deal upon one subject, he may have been carried away by trying to throw an enormous deal of unity, uniformity, and order into his views with respect to syphilis. The fact is, such men as Rouse and Brown in Scotland

aim at simplification. Brown said it was a sthenic disease, or that it was an asthenic disease. Rouse said that it was inflammatory or non-inflammatory, and so on. So you find Mr. Hutchinson addressing you in this way, "with the desire"—I use his own words—"to simplify and make more orderly one general view of the subject." Now, in simplifying the subject, we must be very careful to adhere exactly to the pathological facts that lie before us, especially in practice; and, in the investigation of the disease, uniformity may, perhaps, degenerate into a little confusion, and we know that the way in which certain diseases have been divided for us lightens our task in practice. We follow those who have examined and studied the disease and divided it, and we follow it in our practice as far as we can. Hence, *in limine*, I would say it is rather a heavy task to attempt to simplify this subject to any very great extent. But, rather than take a general view of syphilis, rather than express any opinion of my own, I will pay due respect to the address itself, and endeavour to review it. I find that it may be divided into a certain number of heads: first, one virus; secondly, syphilis is a special disease or a special fever; thirdly, there is such a thing as cell-growth in syphilis which may be followed by phagedæna; fourthly, syphilis presents analogous features at different stages; fifthly, tertiary symptoms are local sequelæ; sixthly, certain contrasts exist between inherited and acquired syphilis; and lastly, syphilis is not convertible into struma. I shall not take the liberty of alluding largely to every one of these points; I will only hint at a few of them, and endeavour not to trespass too long on your patience. First, as to the one virus. Here our distinguished friend has gone a step backwards; he seems to have wished to revive the opinion of Hunter, Ricord, and others respecting that one virus; and in this respect he has had a very great supporter in Sir James Paget, who also concurs with the opinion of Ricord, that there is but one virus, but that it will act differently according to the individual upon whom it works. That was Ricord's view; but he was too busy in separating the virus of syphilis from that of gonorrhœa to pay much attention to what might have been said with respect to the virus itself. He separated, as Hunter had confounded, the two. Several other opinions have been formed, in order to get rid of certain difficulties in the way. I see that Mr. Hutchinsou has taken refuge respecting the differences in the virus in germs. He says, in one portion of his paper, that, if a syphilitic secretion contain the germs

of the actual disease, it will convey it; but, if these germs be absent, it will not. In another part of the paper, he accounts for the soft sore in this way: a truly syphilitic ulceration may become phagedænic, and pus, which is in some degree spoiled, may be the origin of the soft sore. There are various ways of accounting for that soft sore. Why take refuge in those germs, which, I suppose, Mr. Hutchinson has no more seen than either Pasteur or Lister? At all events, there was that supposition of my friend Clerc in Paris, that a soft sore was simply the descendant of one that had been inoculated, and that the sore inoculated upon an individual already having syphilis produced a kind of hybrid afterwards. This was a supposition and a theory. But, after all, why quarrel with the dualists? There are very few books teeming with as much clinical and pathological truth as that of Bassereau, which has stood the test of twenty-four years. The book was published in 1852. I will no further allude to it than to say that it did put upon pretty safe ground the view that the soft sore had existed at all times, and had been confounded; and that eventually, at the pestilence of 1694, the soft sore really made its appearance. It would be out of place, however, to discuss the matter here. Then we come to the question of the specific fever. It is no wonder at all that Mr. Hutchinson should have established his analogy. I am glad to find that he is so true to his colours; because, several years ago, in another place, I had the pleasure of being present when he propounded that view. He certainly might well do so, because, in a physiological point of view, the resemblance is very great. There is only one of these specific fevers, however, where the analogy is quite correct—that is variola—because there we have an original sore; the difference being, however, that variola can be caught by a tainted atmosphere, whereas the other cannot; and, besides that, there is, which is a little inimical to the analogy, the hereditary transmission afterwards. It is plain that there are certain analogies in disease; but why should we force them. Why should we not take one as it is, and the other also? Analogies are very useful in certain respects, but not in all. Then there is a subject which Mr. Hutchinson has investigated with great talent. He has shown us well, as has been stated by Dr. Wilks, that there is a peculiar growth, a peculiar deposit of masses which takes place in syphilis, and which is peculiar to that complaint. We see it at first with the hard chancre, we see it afterwards with an ulcer; we see it in the nose, in the gummata, and so on.

Mr. Hutchinson says in his paper, "I am not speaking of inflammatory processes, but of cell-growths;" and that we all acknowledge as the peculiarity of the complaint. But here the author has run a little too far respecting the manner in which these masses may be destroyed, and he has indulged in a very lugubrious picture of phagedæna. I can quite understand that, by the power of the organism, such growths should disappear and become absorbed; in fact, we are all aware of that most astonishing thing, how the very hard mass surrounding a hard chancre will, after a certain time, and by treatment, entirely disappear; but we should recollect, also, that another process may take place: it may tumble down; an inflammatory process may take place, and then phagedæna may occur. Mr. Hutchinson has ventured upon an expression which certainly was very bold, that syphilis is the originator of all phagedæna. This is going somewhat far. I must say that I have seen, in the course of twenty-five years, a great many very bad cases, both in hospital and private practice, and phagedæna has been rather the exception than the rule. I do not allude to those unfortunate beings in hospitals in whom neglect, poverty, and want of cleanliness, bring on the most destructive processes; but I do not see why we should put at the door of syphilis, which has enough to answer for, all the mischief which may be brought on by phagedæna, to such an extent as to say that when the wizard of syphilis evokes the monster of phagedæna the part is lost. I must say that it often happens that such results are by care avoided. Mr. Hutchinson has been particularly lucky in his analogies respecting the different features which we see in the stages of syphilis. I must adhere, however, to what I look upon as stages, rather than put the whole disease into one lump, as he wishes. He has pointed out that there are different parts which are gradually affected, as the skin, mucous membrane, and so on; and he has shown his analogies wonderfully; but, having in some degree ignored the tertiary stage, he has to some extent overlooked what he himself stated; and that is that, in the secondary stage, there is the peculiarity that masses that are deposited will become absorbed, whereas, in the third stage, these masses are most likely to run into very destructive inflammation; therefore, when he speaks of the great analogy existing between the secondary and tertiary stages, he very wisely also points out that there are those differences; and it is exactly those differences, as they affect practice, that we principally look for in this paper. Now we come to the sequelæ—the tertiary

symptoms; and this is a question which has been treated in his usual masterly manner by Sir James Paget; that is, the question whether we have on one side a blood-disease and not on the other. It appears to me that Mr. Hutchinson has relied a little too much on symmetry; in fact, if he have any failing, that is his failing—he wishes to bring everything into mathematical order. This symmetry respecting syphilitic phenomena, to my mind, does not exist. Recollect what we see every day. First, we see a chancre that is uniform, at all events. Then we have rashes; recollect the roseola—recollect the little psoriasis, the little impetigo scattered all over the body; then we have a sort of glandular enlargement, which may be very unsymmetrical. In fact, the symmetry which seems to be the basis of his theory does not seem, to those who examine the question without any preconceived notions, to be of much importance. Then comes the want of symmetry in the tertiary symptoms. As it is said by Sir James Paget very properly, the disease is on its decline; it no longer has a firm hold on the patient, and there are only some points here and there which show a certain amount of disposition to take on a pathological state, and here the remains of old scars have been called in to prove that there may be a renewal of the disease that had once existed. If I may be so bold as to criticise anything that has fallen from Sir James Paget, I would say that the simile respecting scars of leech-bites does not exactly tell on a lurking pathological process of a secondary kind, which might have been going on in the cellular tissue at a deeper portion, and which, under certain excitement, afterwards recurred. I am sorry that the leech-bites gave Sir James Paget any trouble, but they evidently were not of the pathological description belonging to the remains of a secondary eruption. The great contrast between acquired and inherited syphilis has been touched upon in a masterly manner. That contrast was quite congenial to Mr. Hutchinson, owing to his great labours respecting interstitial keratitis. Here he was at home; and we all agree that his labours in that respect have been of enormous use to our knowledge of the complaint. It is an extraordinary thing that children who have suffered from syphilis should remain in the state of latency so long, and then have interstitial keratitis shown afterwards. The only passage that I did not exactly understand was that which stated that such keratitis had been noticed by Mr. Hutchinson where he had been able to trace infantile syphilis, but that he had seen that same keratitis also

in people who had never had the complaint. This would seem to require some little explanation. The last point is extremely simple, and it has been touched upon by other speakers, whether syphilis is convertible into struma. It has been justly said by Mr. Hutchinson that disease itself will weaken the individual very much, and that treatment will weaken him; but it should never be forgotten that syphilis will come down upon individuals of different kinds; and if they should be already prone to certain strumous dispositions, of course it will be shown afterwards. I will allude to hydrocephalus. I have seen many children who have lived after infantile syphilis with marked symptoms of hydrocephalus. I recollect three or four boys whom I was able to follow up to the age of thirteen or fourteen, who had very big heads. Many people would at once look upon this as a strumous deposit; whereas those who were acquainted with the history of the case, knew perfectly well that it was syphilis. Before I sit down, allow me thank Mr. Hutchinson for the excellent manner in which he has introduced this discussion.

Adjourned Discussion, March 7th.

Dr. BROADBENT, after some preliminary observations, expressing his assent to the doctrine of the unity of syphilitic virus, said:—Respecting the relations of syphilis and scrofula, my experience is simply confirmatory of that of all previous speakers, that there is no direct relation between syphilis and scrofula; that scrofula is not simply syphilis in the third or fourth generation. But Mr. Hutchinson invited contributions of experience of the later history of cases of congenital syphilis, giving as the result of his experience, that they were not liable to syphilitic affections of the nervous system, to gummata, or to nodes in any special degree; or to any serious form of disease later in life, and that, in point of fact, congenital syphilis after the early dangers did not render a life less valuable. As to the first of these points, my experience coincides with that of Mr. Hutchinson. I have seen a great deal of syphilitic disease of the nervous system, but very few instances indeed of diseases of the brain in the subjects of congenital syphilis, and those instances so exceptional that they would rather tend to confirm the rule. As regards the liability to gummata and to nodes, and the tertiary forms

of syphilis, I confess I should not quite have come to the same conclusion. Of course, I see these things from a different standpoint ; but I have seen large gummata in the tongue, large cutaneous gummata, and more especially, and not at all infrequently, the peculiar rapidly destructive ulceration of the soft palate and pharynx, which is so characteristic of tertiary syphilis. I should, of course, put these ulcerations side by side with gummata as tertiary lesions, and I should not have been led to quite the same conclusion respecting their infrequency. But the most important fact that Mr. Hutchinson stated was, that these cases of congenital syphilis are not liable to any serious forms of disease in after-life, and that, in point of fact, they will enjoy (I suppose according to his idea) an average longevity. Now, it has struck me, and this matter has occupied my attention, that the syphilitic physiognomy, the usual signs, in fact, of congenital syphilis, are very rare in and after middle life. My own experience is that they are very seldom seen, and I should venture to say from my own experience, that somehow or other these signs of congenital syphilis do disappear before or at middle life, from some cause or other. Mr. Hutchinson stated that in the cases which he has watched from youth upward, two of the patients had died of contracted granular disease of the kidney ; but this he attributed to the prolonged administration of iodide of potassium. I am in the habit of giving iodide of potassium in large doses, and over long periods ; and I have only seen one case of contracted granular kidney after the administration of iodide of potassium, and in that case another known cause of the disease, namely, frequent pregnancies, had been in operation. Unless, then, Mr. Hutchinson has very strong evidence that iodide of potassium is capable of giving rise to this disease, I should myself be disposed to attribute the contracted granular disease of the kidney, of which the two patients died, to the congenital syphilis. We are all familiar with the fact that cases of contracted granular kidney, to an extreme degree, occur early in life, and before there has been time for the operation of the usual causes of this condition. It may be a matter of coincidence, but I have seen two cases in which there was this contracted granular disease of kidney early in life, with no history likely to lead to this condition in subjects of congenital syphilis. I think, perhaps, also that some of these may die off from amyloid disease. Dr. Fagge, at the last meeting of the Society, stated that the only two recognised causes of amyloid disease were syphilis and suppuration. I think

most physicians here must have seen instances of amyloid disease early in life, in which there had been no suppuration, no disease of bone; and if Dr. Fagge's dictum be true (and I will pay great personal respect to it, for it would not be lightly uttered), then we are left to the conclusion that congenital syphilis must be a cause of amyloid disease, which will carry off some of these cases. Of four cases of amyloid disease of the liver and kidneys in children that I have seen, one was distinctly associated with congenital syphilis. I come now to the question of the relation of syphilis to the specific fevers; but here I must ask permission to circumscribe somewhat the class within which the comparison should be made. There never has been any doubt that syphilis was a blood-poison, and that therefore it must present analogies with other blood-poisons. But if we get no further than that, we shall not have gained much ground. Among these blood-poisons, however, there is a well-known class, which stands limited by certain common characters—the class of fevers. These characters are—as is very well known—first, that the poison is introduced from without, and not generated within—the poison is derived from a pre-existing case of the disease; secondly, the poison is reproduced in the individual, who therefore becomes a source of contagion; thirdly, these fevers have a very definite course and duration; and the fourth important characteristic is, that they occur only once during life. It is to the diseases comprised in this group that syphilis is, I believe, compared by Mr. Hutchinson. And this would exclude from consideration pyæmia, mentioned by Sir James Paget at the last meeting, except for the purpose for which he introduced it, as an illustration of a blood-disease, symmetrically developed in the first instance, and becoming later on unsymmetrical in its manifestations; but it excludes ague (which he also mentions) completely. Ague has nothing in common with the class of diseases of which I have spoken. If the affinity of syphilis with these diseases can be established, we do make a very important step; we place syphilis in its true relations, and we have in syphilis, with its slow and deliberate processes, an opportunity of studying, as I have already said, the invasion of a blood-poison much more carefully than in the more rapid course of fevers. There is only one of the conditions which I have mentioned, which syphilis fails to fulfil, and that is its definite course and duration. If we make the tertiary symptoms a part of the syphilitic process at all, then, I think, by scarcely any permissive latitude can we bring syphilis into the same

group with the well-defined fevers. Mr. Hutchinson limits the febrile stage of syphilis at which we have a blood-poison to the second stage; and he places the tertiaries among the sequelæ, because they fail in symmetry, they break out on the application of external violence, or on other exciting causes. The whole question is, whether the tertiary stage of syphilis can properly be considered as sequelæ. We have heard from Sir James Paget that well-characterised blood-diseases, such as pyæmia, in their later stages cease to be symmetrical; and that under these circumstances want of symmetry does not imply that the disease has ceased to be one in the blood. And there are blood-diseases, such as gout, in which this symmetry is as a rule wanting. But the tertiaries of syphilis cannot very well be compared with the sequelæ of other fevers. There is something specific about tertiaries; they come from syphilis and from nothing else. Now, we cannot say that of the sequelæ of other fevers. With regard to the instances given by Sir James Paget of thrombosis, necrosis, and the like, there is nothing specific about these. If we look through the whole range of sequelæ of acute specific fevers, we find it very difficult to produce a single parallel to a tertiary manifestation. We cannot consider the scrofulous manifestation following measles, or the purulent catarrh, as semi-specific, in the same sense as the tertiaries of syphilis. Perhaps the only illustrations we can find are the parotid bubo of typhus, which is often unilateral, and the renal affection of scarlet fever. Perhaps the recurrent glandular affections and recurrent eruptions of scarlet fever, and even the most striking examples of kidney-affection in scarlet fever, would be rejected if we took Dr. Wilks's view of the matter, and considered it merely as scarlet fever attacking the kidneys instead of the skin or in the throat. As to Mr. Hutchinson's hypothesis, that the tertiary manifestations may be re-grown in deposits or remains which were laid down during the secondary period, I am hardly prepared to give an opinion; and I would ask Mr. Hutchinson in reply to develop it further. I think that this similarity of histological product has been pushed too far, and that we must interpret histological structure by clinical history; and the clinical history of these tertiaries is so different from the clinical history of secondaries, that I think, considering the limited range of histological variation, we should not attach very much importance to the similarity of the gummata in their structure with such secondaries as we know, and with the structure in the primary chancre. And

if we make too much of this similarity, we are leaving out of sight another very important characteristic of the syphilitic process—the process of destructive ulceration. But there are other difficulties in the way of accepting that the tertiaries are the sequelæ of the syphilitic fever. There is the fact mentioned by Mr. Berkeley Hill, substantiated by my own experience, that tertiaries are much more likely to occur in cases where the secondaries have been very slight. Another objection has also been advanced; namely, that a parent who has reached the tertiary stage of syphilis may have a child who shall have syphilis, but shall begin with the secondary stage. If a disease is communicated in any other than as a mere tendency or proclivity, it can hardly be transmitted otherwise than as a blood-disease. I may here take the opportunity of asking whether infantile syphilis, or rather hereditary syphilis, is so severe and fatal because of the infancy or because of the heredity. There is still one other difficulty which I find in accepting this idea, that tertiaries are simply sequelæ; it is that of a woman may become syphilitic by contamination of a syphilitic fœtus; that under these circumstances the woman may have no secondary syphilis whatever, but go straight to the manifestation of tertiaries. My own observation leads me to the opinion that a woman may get syphilis through the fœtus, and that in these cases the syphilis runs a very different course from that which it runs when the woman is infected directly in the usual method. I have very fully set forth my difficulties in the acceptance of the idea that tertiaries are mere sequelæ. I have done so, not because I object to the theory itself which places syphilis among the fevers, but because I have great confidence in that theory, and desire to see the difficulties removed. I have, indeed, so firm a confidence in this theory, that upon it I base the opinion that we may be called upon to revise our do-nothing treatment of fevers from facts which we see in the history of syphilis.

While, again, in the ordinary sequelæ of fevers there is nothing which can be called specific, in the results of fevers there is something which is markedly specific, and that is, immunity from subsequent attack. Sir William Gull long ago showed that this immunity could not be explained on the hypothesis that the fever-process had exhausted fermentable materials, but that it must be due to some change in the tissues themselves. I think it is not unlikely that the specific immunity of fevers may be found to be represented and carried further in the tertiaries of syphilis.

Dr. BUZZARD.—I propose to allude to two points in Mr. Hutchinson's address—first, as to the occurrence of visceral lesions in secondary syphilis ; and second, the hypothesis that tertiary gummata are local regrowths in formations left over from the exanthem stage. With regard to the first of these points I shall confine myself to syphilitic affections of the nervous system. It must be remembered that long after various forms of nervous lesion had been referred to syphilis, it was supposed that, in every case, the mode in which the nervous system was affected was indirect, and by means of some disease of the bones of the skull. It was only in process of time that it became known that syphilis could cause directly disease of the membranes, of the interstitial neuroglia, and, still later, of the blood-vessels of the brain. Although it is now a great many years since syphilitic affections of the nervous system were described as being capable of happening during the secondary stage, the idea of the frequency at all events of such occurrence has not penetrated largely the minds of medical men, at least in England. They are still very generally supposed to be confined to the tertiary stage. Since the last meeting of the Society, I have examined my notes of a hundred cases of affections of the nervous system, which I diagnosed to be due to syphilis. The mean age of the patients was thirty-five years. I put in this list no case in which any disease of the circulatory apparatus or kidneys could be discovered by examination, and in only two instances was there any history of antecedent injury. Of these hundred cases, I was able to obtain the history of the probable date of infection in eighty-three ; and, out of those eighty-three, in fifty-six the nervous affection occurred at a date longer than five years after the infection, and twenty-seven at a date under five years from the infection. Dividing the period of twenty-five years (which embraced the longest interval that was ascertained) into periods of five years, the first period—that under five years—included a third of the whole number of cases. The next period, namely, that from five to ten years, embraced twenty-three cases ; and that from ten to fifteen years, twenty-five cases. Then they fell rather rapidly ; from fifteen to twenty years the number was only six, and from twenty to twenty-five years only two. Under two years the number was six ; under three years, four ; under four years, six ; and between the fourth and fifth years the number of cases was eleven. These were cases of hemiplegia, of convulsive disorder, and of paraplegia. There is a point of interest that arises here. I found no case of

optic neuritis occurring in any patient at a date less than three years from the period of infection; and out of these hundred cases, I found optic neuritis, or atrophy of the optic discs, arising from optic neuritis, in no fewer than fifteen cases. There was paralysis of muscles of the eye in twenty cases; there were appearances of disseminated choroiditis in five cases, and old iritis in two. In the period from three to five years there were four cases in which, combined of course with other symptoms, there was optic neuritis, three which presented paralysis of the eye muscles, and one disseminated choroiditis. In reference to the character of the affections in these very numerous early cases of syphilitic nervous disorders, there was nothing very remarkable. I can only speak of the symptoms they presented, because none of them died; but, judging of the pathological condition from what one could observe of clinical signs, there was no marked difference between these cases of early occurrence and those of persons who showed nervous lesions at a very much later date. There were, in a few instances, some exceptions to that. There is a peculiarity in some of these early cases of syphilitic nervous affection. The patient complains of fainting, and you see a condition of contraction of the veins where they are superficial enough to be visible. You see the pupils becoming large, and you find that the action of the heart is accelerated. These are symptoms of irritation of the vaso-motor nerves. I have only found them in cases of early syphilis. I have seen a condition of mental excitement, approaching mania, in several cases in very early (secondary) syphilis.

As regards the second point that Mr. Hutchinson mentioned, that tertiary growths represent local renovation of long-existing germs, I should say that I incline to agree with him, and also with the opinion of Dr. Wilks. It is most likely that the lines of the future gummata are laid down during the exanthematous stage. The absence of symmetry is peculiarly evident in syphilitic nervous lesions. The gummata in tertiary syphilis occur almost constantly on one side or other of the brain; and if there should, at first sight, appear an exception in the case of optic neuritis, which is almost always double, it should be remembered that optic neuritis is produced by a single tumour acting upon a single hemisphere. I have seen numerous cases in which hemiplegia or paraplegia has recurred more than once. This would suggest, not the continuance of a blood-disease, but a local deposit which every

now and then has led to renewed action. The absence of pyrexia, which is so significant a part of the second stage of syphilis, is another symptom. Did I not shrink from rushing in where Mr. Hutchinson and Dr. Wilks have feared to tread, I should suggest that we might even hazard a guess as to the particular tissue in which that local deposit occurred.

The researches of the last few years tend to show that the lymphatic system is much more widely spread than was formerly supposed. In addition to the long-recognised vessels and glands, not only the serous sacs (pleura, peritoneum, &c.), but also, amongst other structures, the peri-vascular canals, the fissures between the canaliculi of the testis, and those ill-defined pouches of loose connective tissue which allow a muscle to glide over periosteum or a subjacent muscle (I do not refer to the true bursæ mucosæ) have been discovered to be lymphatic spaces, whilst the tonsils are collections of follicles representing lymphatic gland in its simplest form. The frequent occurrence of gunmata in situations of this kind is notorious. I might suggest that, during the exanthematous period of the disorder, germs, or potentialities of disease are imbibed from the tissues, are carried into the various portions of the lymphatic system, and there lie inert for a time, until some circumstance, with the nature of which we are not acquainted, calls them into action; and that then the outgrowth occurs.

Sir W. JENNER (who was requested by the President to address the Society) said: Having heard but little of this discussion, I am sure the Society will excuse my not entering fully into the subject. There are one or two points, however, to which I would refer. I must own that, although I have seen a good deal of what I would call acute specific diseases, I am not acquainted with that marked symmetry which has been alluded to. Take the eruption of typhoid fever. I know nothing symmetrical about it. I should say that want of symmetry is one of its characteristics. Ordinary psoriasis (such as I can produce in some persons at will by irritating the skin) is symmetrical; yet I do not think it is a blood-affection, but rather that it is a potentiality of developing tissue. I cannot see that symmetry necessarily arises from a blood-condition; nor can I see that certain diseases in which the blood is damaged are necessarily attended with symmetry.

There is one specific disease, one acute febrile affection, to which,

without entering into the question of tertiary conditions, I would call Dr. Broadbent's attention, in which there do seem to be sequelæ of a very peculiar nature which do not appear to be mere accidental disturbances of nutrition. Diphtheria is unquestionably an acute specific disease, having its origin in a special poison. It runs its course, and then, perhaps, in a fortnight or three weeks, or a month, you see results that you would never dream of referring to the attack of diphtheria except by a series of observations and by a long process of reasoning. The child begins to see badly, and you say that the muscles of the eye are affected through the nerves; then fluids return through the nose; then the child begins to stagger. There, it seems to me, you have the sequelæ of fever, not in the sense of mere disturbance, but as a part of the residue of the disease itself; something quite different from the scarlet fever affecting the kidneys or diphtheria affecting the brain, but something special. Then I would speak of congenital syphilis. Of course we all see these internal syphilitic affections; but surgeons, perhaps, are better acquainted with them, as a rule, than physicians; congenital syphilis, however, does come under the observation of the physician, and I cannot help seeing here something that is not blood, but something in which the blood may, and probably does, share the injury. I see the father, as Dr. Broadbent has put it, the sperm, something anterior to the blood, the sperm on the one side and the germ on the other, when it is a mere cell, going on to division after division, and yet every one of those particles partaking of the syphilitic character. And then I see the first development in the tissues, in the tissues which are being formed before and in common with the blood. It appears to me that these primary cells are the real seats of the change. It seems further, as in cancer, that the father or the sperm does give to the primary cells a potentiality of development; and that seems to me to be the key to these congenital diseases; a potentiality of development which shows itself in the syphilitic child at the earliest period, but still goes on until the second dentition and afterwards. We see this potentiality of development coming by the parent to its offspring in all the tissues visible to the eye,—in the colour of the eye, the formation of the pigment, the formation of the whiskers, and so on; and this potentiality is seen not only in formation, but also in decadence. Thus the father, by the stimulus of his sperm, has given to every tissue of the child this potentiality;

even the teeth may fall out early or not, according to the power given to the germ, but not showing itself till late in life. Potentiality of development possessed by the germ if inherited from the mother, or impressed on the germ if inherited from the father, then, and not the state of the blood, not any visible organic change of structure, is the real pathology, it seems to me, of infantile, *i. e.* congenital syphilis.

Dr. MOXON: Before proceeding to any more special remarks, I should like to felicitate Sir William Gull and myself, and those who took the same view, upon the very distinguished support that we have just had in regard to our view on the pathology of cancer, which I rather understood Sir William Jenner to oppose on a former occasion, though I now understand him to say with us that it fixes itself upon kinds of tissue rather than that it belongs to the blood. That only in passing. I will now address myself to the matter before us. Mr. Hutchinson has asked specially for facts. I thought I had brought with me some facts noted down that I had collected; they were not exactly what Mr. Hutchinson asked for, such as were required to support or oppose very special lines of theory which he had spun out; they have no bearing either for or against such theories, but have only a general interest to the profession (so, at least, I thought), representing my own experience with regard to the time at which visceral syphilis is fatal. I found in the course of fifty-six inspections upon persons who had died of visceral syphilis, that the average period at which death occurred was as early as thirty-seven. It is worth while to have that fact before us, if only with reference to prognostic uses, as in the case of life-office work, though it may have also a bearing upon general prognosis. My own experience has led me to think that, if a person is once tainted with visceral syphilis, his future is a short one. I do not wish to produce any melancholy effect upon the mind of the meeting, I was only thinking of my patients, who are always in my mind. When once visceral syphilis has declared itself, as Dr. Buzzard has mentioned, sooner or later, or rather sooner than later, the end must come. This experience, however, is an experience obtained by the treatment of the disorder only by iodide of potassium, or nearly only. Latterly I have found, in dealing with visceral syphilis, that one can temporarily relieve the disease by iodide of potassium; but that a mild course of mercury is a good

thing to effect a cure. If that be the case, it seems to me to have a certain bearing upon the question whether syphilis is one and indivisible, or whether it is to be divided into two distinct things. When I was first taught about syphilis, one of the distinctions between so-called tertiary and so-called secondary syphilis had reference to their medication. We were taught that iodide of potassium was the proper remedy for the tertiary stage and that mercury was the proper remedy for the secondary stage. It was almost the only main point in regard to the practical distinction between the two. Indeed, as to all other points of distinction I was rather of Mr. Hutchinson's opinion and thought they were comparatively trifling. Mr. Hutchinson is supposed to take quite a different view of the subject from that which he really holds. Let me read what he says, speaking of the differences between secondary and tertiary syphilis: he says, "Whilst, however, their similarities are marked, so also to some extent are their differences." Then he proceeds to state those differences; and with regard to the second one he says, "The second difference is that a spontaneous tendency to resolution of the new growths and to absorption is constantly witnessed in secondary syphilis, whereas it is exceptional in all tertiary products. It is, however, by no means certain that spontaneous disappearance does not often occur in the case of tertiary growths. Still, it may be fairly granted that a proneness to persist to grow, to spread, and to contaminate adjacent parts, is far more frequently witnessed in the tertiary than in the early formations"—thus putting the thing in a sort of contemptuous way upon its legs again. As to the other criterion, he says, "First, and by very far the most important of the differences between the stages, we place one which has been already mentioned, a tendency to general and symmetrical development in the secondary stage, and to local, restricted, and unsymmetrical formations in that of sequelæ." But listen to what follows:—"This, however, after all, is merely a question of abundance, and not so much of character." It seems to me that when Mr. Hutchinson has said that the difference between the symmetrical and unsymmetrical eruptions, after all, is only a question of abundance, and not of character, he surrenders the whole position, and that Dr. Wilks was right in claiming to belong to the same school of thought in that matter as Mr. Hutchinson. I do not think all has been said about symmetry that may be said. It is admitted on all hands, I think, that secondary syphilis comes out in

a burst of hundreds, if not thousands, of blotches; and since it is the usual experience that tertiary manifestations are one, two, three, or several, it seems to be a matter of common sense that a symmetrical figure like ours would, if blotched on a thousand places, be somewhat alike on the two sides. I will mention an experience that occurred to me last Sunday. I was walking out with a friend, and I had an umbrella, but he had not. Before we got very far I cheered him up—for he was looking glumpy—by pointing out that his figure was symmetrically spotted with rain. He was not a pathologist, and he did not seem to see the joke. I then pulled down the umbrella and showed him the radial symmetry which it exhibited—the beautiful radial symmetry of the rain all round in the radial segments of the umbrella. He had not been bred in the mysteries of fluid pathology, and was rather out of temper and was rude enough to say “Stuff!” He thought the symmetry was the work of the umbrella maker, and not of the rain. Well, there is something in that. One would expect that you would be pretty much alike on the two sides in a rain-shower or in a scatter of syphilis over the surface. You would be symmetrically spotted, not, as Mr. Hutchinson rather satirically says, with a Dutch gardener’s symmetry, although I was a little taken aback at finding a mind of Mr. Hutchinson’s precision thus slighting exactness—not the symmetry with which nature gardens the two sides of one’s face so Dutchly equally—but a symmetry sufficient, shall I say, for the purposes of poetic pathology. I may call this the fallacy of universality. But I shall be told that this does not apply to the really singular and beautiful manifestations of symmetry which Mr. Hutchinson mentioned when he pointed out the symmetry of secondary tonsillitis, of secondary iritis, and of secondary keratitis—that is, if keratitis is secondary, though as it may come in the thirty-fifth year of a man’s life it is very late; and it struck me that if Mr. Hutchinson allows a secondary at thirty-five he is hardly in a position to wonder that the tertiary does not appear in the bodies of persons with hereditary syphilis, seeing that if secondary comes at thirty-five, then when the tertiary comes I should think the patient would be dead and safe. Assuming that iritis, keratitis, and tonsillitis are really secondary and also symmetrical, I admit at once that what I call the fallacy of universality would not apply. But something equally simple does. It strikes me that the reason why keratitis is so symmetrical is because there is no more cornea to be affected. If instead of two

eyes we had five, like the scorpion (if the scorpion has five), or if the Society can imagine a syphilitic butterfly with twenty-five thousand eyes, it would have twenty-five thousand patches of keratitis. Or can the Society follow me in this flight?—Suppose that poet whose works I once read who was so struck with the beauty of nature that he wished he was all one great eye that he might see at will through every pore—suppose that poet misled into a *faux pas* (for poetic feeling produces no little of the reality as well the pathology of syphilis)—suppose that the unfortunate monster caught syphilis—then I suppose his universal cornea would be liable to a universal keratitis. In short, it seems to me that the case of the cornea is much the same as the case of the liver. It is true the liver is all in one piece and the cornea in two; but if you are not surprised that the liver is universally affected, what occasion have you to be surprised that the cornea, which happens to be in two pieces, is also universally affected? I name that fallacy the fallacy of totality. Then there is another fallacy in this discussion as to symmetry, but the display in which it appears is so bright that I approach the distinguished authority opposed to me with a great deal of diffidence and with an explanation as simple as “a smooth stone out of the brook.” Sir James Paget brought forward, as instances of asymmetry, the suppuration of a rib and the inflammation of a vein after typhoid fever. Sir W. Jenner has pointed out that the typhoid fever eruption itself is not symmetrical; so unsymmetry of so-called sequelæ would not be surprising. Sir James also mentioned the neuralgia of ague. But it is surely on the face of the question that, if you have only suppuration of one rib, it cannot be symmetrical; it takes two ribs to make it symmetrical. Again, if there be inflammation of a vein of only one thigh, you cannot have it symmetrical; and as Sir James Paget did not show any reason why there should have been two attacks and not one, and as the question of there being more than one precedes the question of how they should be arranged if they were present, I think we may say that the question as to symmetry is not even approached in such instances. I call that the fallacy of unity. A gun may be wonderfully well loaded and you may be a good shot, but if you fire out of range you are sure not to hit the mark. My next proposition may perhaps surprise some members. The most remarkable examples of symmetry are in tertiary syphilis. Here, again, I shall be able to bring some facts for Mr. Hutchinson. The first fact will be a case at present under Mr.

Clement Lucas, my able colleague at Guy's, in the person of a woman who has a strictly symmetrical pair of tertiary ulcers, one outside each ankle. He has also a case in which there is a strictly symmetrical pair of ulcers, one on the top of each knee; and also a case where, four years after the infection, the palms of the hands are the subject of symmetrical psoriasis. Then there was a very remarkable case inspected by Dr. Goodhart, of a woman who died of brain syphilis. We found on the pneumogastric roots on each side, where they are in union with the lateral horns of the fourth ventricle, a mass of tertiary syphilitic gummata about the size of a broad bean; it was a most curiously symmetrical development in tertiary syphilis. In those four cases no doubt the fallacies of universality, totality, and unity, do not apply; and I should challenge any one who believes in the exceptional symmetry of secondary syphilis to bring forward facts similar to those in regard of it. I can get more, if he does, to which those fallacies do not apply. These are examples of a true symmetry. They belong to a set of examples of symmetry due to local peculiarities of exposure, &c., some of which were brought forward by Sir James Paget, though they seemed very curious as examples of blood-disease. Pityriasis versicolor was slightly mentioned by him, for instance; but, of course, this is no blood-disease any more than itch is a blood-disease, which is also truly symmetrical; so, too, is trichiniasis curiously symmetrical, and it is not a blood-disease, although the trichinæ no doubt get a lift in the blood as you may in a tramcar. I now pass on to the second point which has been in everybody's mouth who has spoken on the subject—Mr. Hutchinson's great theory as to the relation of the secondary and tertiary stages. This discussion is one worthy of the Society, and I trust I shall not be thought trifling with your time if I take a moment or two longer than my proper share. It will be seen that the division is a great one. We have had one of the most eminent and accomplished surgeons in this or any other day state views in this Society which are directly opposed to no less an authority than the Royal College of Physicians. I see Dr. Pitman before me, and he will no doubt correct me if I am wrong when I state that the College of Physicians is dead against Sir James, because in their nomenclature syphilis is put, not in Class A, general diseases of the blood, but in Class B, of which I do not pretend to give a definition, perhaps Dr. Pitman will. I may now be permitted to state my own view of the

question whether syphilis is a blood-disease. I do not believe or accept the belief that syphilis is ever a thing of the blood especially. I do not mean to say that blood from one syphilitic person will not affect another. In that case it must be *in* the blood. But then we may catch a physician in a railway train, yet you would hardly say that medicine belongs to railway companies; it is merely a matter of transit. I now pass on to the theory as to the relation of the secondary and tertiary stages:—"The importance of all facts or arguments for or against the belief that the gummata of the tertiary stage are purely local and result in renewed growth in formation left over from the exanthem stage." Mr. Hutchinson does not seem to have the remotest conception of the possibility of any other belief. I have a distinct other belief, which I hope to be able to show the Society is capable of being held in a reasonable way. First of all I ask the Society to allow me to state my belief that syphilis is never a thing of the blood only; and my next point is that all febrile disorders, when they strike virgin ground, rage with comparatively great freedom. I will only mention the development of smallpox and measles among races new to it, and bring forward the fact that the powerful and muscular suffer most from the fevers, and that most febrile disorders give immunity, which we may interpret simply by saying that when once people have gone through the febrile course they are no longer susceptible to its influence. I agree entirely with Mr. Hutchinson in the view that syphilis is a fever. I am not sure that I am right in the impression that rests on my mind, but I thought it was Linnæus who first made out that point. It would be well if it were, because botany affords a beautiful parallel for it. I read in the 'Linnean Transactions' nine years ago of a certain palm-tree of the Seychelle Islands (*Lodoicea Seychellarum*) which takes four years for a bud to form, two years for it to blossom, and three years to perfect the fruit. The blossoming is slowed, and in the same way we may say that syphilis is fever diluted with time so as to be cooled and slowed. If you could shorten it, as in the case of those photographic puzzles, you would get it out as a fever. I say syphilis is admittedly a fever. It will follow the same laws as the rest of the fevers; and the great law of fevers in reference to liability to attack is, as I have said, that what is fresh is liable to attack. The next point is this. When a woman, years ago, has had syphilis, her syphilised tissues have been so changed that they are no longer vivid to syphilis, and when she gets a foetus

(a new mass in her not yet syphilised) the independent growth of new stuff is vivid to syphilis. Thus we find that she carries within her, years after her own syphilis, something which has not had anything to do with syphilis, and is ready to take it on. I do not go with so much faith into the history of those ladies Dr. Broadbent spoke of as he has done. There may have been something in the past which they have forgotten. This new lump, the foetus—which I have heard called osteo-fibro-sarcoma, to all intents and purposes it is a new piece of flesh not yet syphilised, and it takes on syphilis. The placenta is always found diseased in syphilitic abortions. Those who have followed Virchow and gone into this question will say it is not tertiary syphilitic gumma that you get in the placenta. Virchow makes a strong point of that, and says that he has rarely seen tertiary syphilitic gummata. So much the better for my theory. It is said that what you get is fatty degeneration? Pray, sir, what is that? The placenta has no cornea, has no tonsils, no iris; and it cannot have keratitis, or tonsillitis, or iritis. What can the poor placenta have but fatty degeneration? Secondary syphilis of the placenta shows itself in fatty degeneration, and I say this new lump is syphilised and turned out. That will show pretty well why persons apparently free from syphilis should get a syphilitic foetus. I may be asked how tertiary syphilis comes in adults; and here I shall ask the members of the Society to go back to long-buried knowledge, which, perhaps, they never perfectly appreciated, to knowledge which I remember taking in almost with a sensation of anger. I had read the account in Quain and Sharpey of the histology of the skeleton, and got through the lacunæ and canaliculi and Haversian systems and so on, and then I learnt that Messrs. Tomes and De Morgan had discovered Haversian spaces, and I was very much disgusted. These Haversian spaces were hollows cut in the adult bone—it seemed most unnecessarily—and then filled up with new, solid, good bone. Then I must do justice to my friend and colleague Dr. Fagge, who about three years ago asked me whether I ever recollected that circumstance, and I must say that up to that time I had not revolved it in my mind. He asked me if I thought it possible that the same thing might happen in other textures, for, in fact, the bone shows well written out in its permanent hard inscription what happens in the history of the softer texture; that soft texture might similarly go through all those changes without our being able to detect them. I am happy to accept that view,

because it suits my present purpose. Let me mention a case that I once saw, a case where the right lobe of the liver was gone and the left lobe weighed as much as the whole liver would have weighed ; there must have been an immense production of new substance. A man in training increases the weight of his muscles and bone, he must have new tissue. You see the line of thought that my theory leads me along. Instead of thinking with Mr. Hutchinson that the developments of tertiary syphilis are due to the breaking out again of fires in already effete hearths, I am inclined to think that it comes in such portions of an adult frame as have arisen afresh recently, after the law of Haversian spaces or after the law of hypertrophy, into existence. They have not had their share of syphilis, and are bound to take it in their turn, just as the syphilitic fœtus is so bound. Whether it be a true view or not I do not mean to pursue it before the Society ; but I will say that the more I have pursued it the more it seems to fit in with many phenomena which raise difficulties in the minds of those who take the fluidist view. I am not fond of hypotheses, and I am at least willing that if Mr. Hutchinson will agree never to say anything more about his theory I will agree never to say anything more about mine. I shall be happy to hand over to him any few facts which I have been able to collect before the Society, feeling sure that he will be able to fit them into the construction which he will finally crown at the conclusion of our individual efforts.

Adjourned Discussion, March 21st.

Mr. THOMAS SMITH.—Mr. Hutchinson very early in his remarks stated that to the annoyance and misery of thousands syphilis did not always end with its apparent death at the end of the secondary stage. But, later on, he remarks : “ the lesions of the tertiary stage by their non-symmetry appear to prove that now at least the blood is not concerned ; ” and as additional evidence in favour of this absence of blood taint in the tertiary lesions he cites their non-contagiousness and the power that is exercised by local remedies in their treatment.

Had Mr. Hutchinson himself attached any real importance to this want of symmetry in the lesions of the tertiary stage in contradistinction to a supposed symmetry in the secondary symptoms I

should have been prepared to dispute altogether this alleged contract between the two sets of symptoms. But since he subsequently admits that "symmetry and no symmetry is a question of abundance and not of character," it is not necessary to discuss this question: since we should all agree that in the early stage of constitutional syphilis the symptoms are more general and more widely spread than in the later stage.

Concerning the non-contagiousness of the lesions in the tertiary stages Mr. Hutchinson admits that there is no direct proof one way or the other, and as I do not intend to take my stand on this point I am willing for the sake of argument to allow that secondary syphilis is non-contagious. For the same reason I will acknowledge that local remedies have some efficacy in the treatment of the tertiary syphilitic lesions he enumerates: viz. palmar psoriasis, lupoid tubercles, some affections of the tongue. Yet I cannot allow that it is usual to see any permanently beneficial result from local treatment in ulcerated gummata, syphilitic ulceration of the legs, in deep tertiary ulcers of the face and nasal cavities; yet in all of these the remedy can be brought into contact with the diseased part otherwise than through the medium of the circulating blood, yet it is only in this latter manner that a cure can be effected.

But what can be said in favour of the view that the symptoms of tertiary syphilis are as a rule the result of a persistent blood contamination? Passing by all that has been urged on this point by others, I would take my stand on a fact that is not disputed by Mr. Hutchinson, namely, that the power of hereditary transmission persists during the tertiary stage; or as he puts it, "the risk of hereditary transmission persists long after the cessation of blood contamination."

Mr. Hutchinson admits a "difficulty in conceiving of this;" to me the difficulty is insuperable in the case of syphilis.

Now, I assume that the terms "blood contamination," blood disease, are employed in their generally accepted sense, and that Mr. Hutchinson means that a man may beget syphilitic children when he himself is no longer the subject of constitutional syphilis. This, I take it, is the plain meaning of Mr. Hutchinson's statement, and this is what I cannot for a moment admit.

I may at once be met by the objection that there are other constitutional diseases, such as gout, which a man may transmit to his offspring, he himself being free from the disease at the time of trans-

mission. Or still more—a man may transmit to his son a something which may give no sign of existence until it becomes gout in the grandson; so that in this last case the intermediate individual (the pestiferous person) transmits a poison or disease that he never at any period of his existence suffered from.

I have taken gout as the strongest instance that can be brought forward in support of the view that a man may transmit a disease to his children, and yet not be himself the subject of that disease.

Now, in relation to the heredity of gout, I believe it can be said with more truth than it can of syphilis that “the germ or sperm does give as it were a potentiality of development in one definite direction which potentiality shows itself at variable periods of life”—these are Sir W. Jenner’s words.

This potentiality of development transmitted with the sperm in the case of gout requires years to breed the fully developed disease, for a man generally grows up into gout or, perhaps, more often down into it, and indeed it may require more years than there are in one man’s lifetime, for in the case where gout survives a generation this potentiality takes one whole lifetime and part of another to produce the actual disease.

In strong contrast to all this are the circumstances attending the hereditary transmission of syphilis, I maintain that from the earliest contact of the parental sperm with the ovum there is distinct evidence of the existence and transference of a deadly disease and what I take leave to call a blood disease—a disease which may destroy the fertility of the seminal fluid, may blight the ovum, may kill the foetus in utero—may attack the newly born infant, may poison the mother through the foetus, in her pregnancy or during parturition.

In support of these assertions it is not necessary for me to do more than mention the notorious infecundity of syphilitic marriages, the abortions, miscarriages, and other catastrophes which may occur.

In all these particulars there is a striking contrast to what occurs in gout, nor will the same explanation suffice. In syphilis the germ or the sperm transmits something more than a potentiality of development; it transmits a real disease directly continuous from parent to offspring.

I would direct attention to one or two other points of difference between the two diseases: hereditary gout may miss one generation and appear in the next; does hereditary syphilis ever do this? Who ever heard of a mother contracting gout by bearing in her womb

the child of a gouty father? Who has not known of syphilis communicated in this way; and how is the disease passed to the mother? through the blood of the foetus: in the placental villi to the blood of the mother in the uterine sinuses. If this be not a blood disease in the reasonable sense of the term, what is a blood disease? It is an all-pervading disease.

I am very anxious that Mr. Hutchinson should not misunderstand me. I quite agree with him that the degree of blood contamination is constantly diminishing, in syphilis, until it comes to a vanishing point, that the risk of hereditary transmission in the later stages of tertiary syphilis is very small; that it is possible for palmar psoriasis, ulcers, and other lesions considered evidences of the disease to outlast the stage of blood contamination. Yet I want to persuade him that the risk of hereditary transmission does not persist long after the cessation of blood contamination; but that the power of hereditary transmission is evidence of the existence of blood contamination.

Mr. Hutchinson concludes his paper with this inquiry—Are there any facts which favour the belief that syphilis continues to be a blood disease after the cessation of all tendency to produce symmetrical symptoms? My answer to this would be, Yes,—there are many recorded cases where parents suffering from gummata, caries, and the later lesions of tertiary syphilis, have begotten or given birth to children who speedily became the subjects of the same disease in its secondary form.

Sir WILLIAM GULL.—I rise to speak on this important subject, because of late years the branch of the profession to which I belong has become greatly interested in the question of syphilis. For too long a time, I think, syphilis was entirely consigned to the tender mercies of the surgeons; but I confess, among the things I have seen and studied in medicine, the history and course of syphilitic infection has seemed to me one of the most interesting. When we consider the origin of the poison (which is no doubt associated with the generative fluids), we might almost predicate of it that it would be a strangely pervading poison. That may not, perhaps, at first strike those who hear me; but I think, if you consider the origin of the poison, you will say that it is most likely we should have predicated of it that it would pervade almost every tissue of the body, and that it would not be limited to the individual, but be

propagated to his offspring. In that respect it is always peculiar. It has always been an interesting point with pathologists to make syphilis one of the fevers. Dr. Robert Williams, in his book on poisons, classes it with typhus and typhoid; and those who have thought most on the subject have regarded it as belonging to the class of fevers. But, though it may be so placed, it is still a peculiar poison. Take the case of variola. A father has variola, a mother has variola, but the children of those parents seem to be quite uninfluenced by the variolous process. That is not the case with syphilis. The same may be said in regard to typhoid, typhus, measles, scarlatina, and, in fact, all the class of the so-called fevers. Therefore, if we are to place syphilis amongst the fevers, it is certainly a peculiar fever in this respect, that it is not limited in time as other fevers are. Mr. Hutchinsou said in his paper that he thought that, if syphilis were left unhindered by treatment, it would be limited in time. I agree with him that in some respects it is limited in time (I have myself noticed such cases) when uninfluenced by treatment, but that must be in a particularly healthy individual, probably young in life and unspoiled by those other diviuties which go with Venus, namely, Bacchus and Mercury. Occasionally it is limited, and most remarkably limited, in time; Nature herself can stamp it out; but that is by no means its universal history. If it be placed among fevers, it is a fever not limited in time as they are, for it can go on, I think, almost *ad infinitum*. We have evidence that a father may beget a syphilitic child when he is past the constitutional condition of syphilis, as has just been insisted on; that a child may be born apparently healthy and may remain healthy for some weeks, and yet after a time, if children are vaccinated from it, they may have syphilis. We have nothing like that in ordinary fevers. A child born of variolous parents is as free from the influence of variola as though they had never had it, and there is no mode, so far as I know, of producing any influence upon it. Again, it is a fever without pyrexia. The febris and the pyrexia have, I am happy to say, long been dissociated in clinical medicine. Then, is it a blood-poison? We have come to the old question of blood-poison again, but I will not take it up. My friend Mr. Smith has just said that he uses the term "blood-poison" in a general sense just as people commonly say "it is in his blood." Then he adds that he thinks it is in the blood proper, that is, in the circulating fluid, and not in the tissues and not in the lymph. He first uses

the term in a general sense, and then in a more or less limited sense. Well, I think syphilis is a flesh-and-blood fever; I think that is its whole clinical history, and that that is the proper expression of syphilis. There is no tissue in which it does not exist; there is no fluid in which it does not exist. We have a proof of its being a flesh-and-blood fever in this, that an apparently healthy child may be born of syphilitic parents, may be vaccinated, and the vaccine lymph may pass from one child to another without conveying syphilis; but, if blood is mixed with it, it has an influence. Mr. Simon will tell us whether that is a fair inference or not. If so, then syphilis continues to be a blood-poison even in healthy children, so that it is a flesh-and-blood poison; it is a flesh-and-blood fever. Then, as regards the tertiary effects, which physicians have most to do with. Surgeons have the primary disease, and largely the secondary disease, and then physicians come to those singular tertiary effects; and there I think I must agree with Mr. Smith that it still continues to be a constitutional affection. I think that syphilis continues to be a constitutional affection through the whole life of the man who has had it. Syphilis once, syphilis ever; syphilis general, syphilis universal, in the man all the time he lives. He may wear it down more or less, still it may remain there. This is very important clinically. You may have a syphilitic patient who may give no evidence whatever of syphilis to the most experienced person. Now, there is no better authority on this matter than my friend Dr. Wilks, to whom we owe a great deal of our knowledge of the effects of syphilis upon the internal organs. I remember a case which I have often quoted, because it shows how little evidence we can have except local conditions to guide us in regard to the after-effects of syphilis. A patient was placed under my care whose nutrition had become extremely faulty; he had lost two stones and a half in weight, and had become more or less generally paralytic—quite different from the ordinary limited form of paralysis to which we are accustomed, as depending upon deposits of gummata. I knew that he had had syphilis, and that from time to time he had had affections connected with that taint. He became very ill and went into the country, where I went to see him, and prescribed accordingly. I was unable to continue my visits, and Dr. Wilks went to see him for me; and he, though he is so apt at all this pathology, could not see, and would not believe, that the man was suffering in any way from the effects of syphilis. If the result of treatment be a proof,

he certainly was suffering from it; for directly he was put upon proper syphilitic remedies, he not only recovered his weight, but his health, and lost all his nervous affections. I believe, therefore, that the effects of syphilis continue, or may continue—I do not say that they actually do—during the whole period of life. Hence Mr. Hutchinson's difficulty, I think, is one that he rather creates for himself. In that respect I agree with Mr. Smith. We see the effects in the hair and in the complexion; we smell it in the odour of the sweat, and that when there is no local development of syphilis. I remember a case of this kind. A man was supposed to be fading away from tubercle; it might be diffused, but where nobody seemed to know; and he was sent to me for examination. I found no tubercle, but I was struck with the odour of the sweat; and I said to Sir James Paget, "I think I can smell tertiary syphilis." We found no gummata, no psoriasis, nothing to show the presence of syphilitic poison in any tissue; but I believe it was there. I remember remarking to Sir James Paget: "If we had found a copper-coloured rash in his palm, we should have been satisfied." We found it nowhere about him, yet the man was syphilitic. This was years after he had the taint. He was immediately put upon the proper remedies for the tertiary taint, and he quickly recovered. I believe, then, that over and above the gummata there is a general condition still remaining, quite bearing out what Mr. Smith has said in reference to there being no limit to the affection of syphilis in the tissues and in the blood. I cannot sit down, occupying the position I do in the profession, without thanking Mr. Hutchinson and those who have worked with him in this matter. I think the whole profession of England are greatly indebted to Mr. Hutchinson, to Dr. Wilks, Dr. Buzzard, Dr. Broadbent, and a number of other gentlemen belonging to this Society who have worked out this matter. I do think it is an enormous advance that we have made in recognising that there is one poison which produces an infinite variety of effects according to the constitution upon which it operates. We have also made a great advance in believing that what are called primary, secondary, and tertiary stages, are artificial divisions, rather than divisions that nature makes. We have learned, I think, in these days, that it does not require a primary sore to infect a second person; that the secondary disease is as infectious as the primary, and is only a general form of the primary; and that in all likelihood the tertiary disease is but a

diminished condition of the secondary, as we see by the children who are born of such parents.

Mr. SIMON.—I rise, sir, in answer to your summons; but, indeed, for many reasons, I would have much preferred to remain merely a listener to this very interesting discussion. On the subject about which Sir William Gull referred to me I have nothing to say; but there are some, in fact there are numberless, parts of the argument which have interested me very much, and if I had expected that you would have done me the honour to call upon me, I would have made a note or two of the points on which I might offer some remarks. But generally, perhaps, I might offer this criticism on Mr. Hutchinson's admirable paper (in the admiration of which, expressed by Sir William Gull, I most entirely concur), that I think it calls upon the Society too much to answer with an absolute "Yes or No" questions that are really to be answered by "more or less." As to the first proposition, that syphilis, when it ceases to be a blood-disease, ceases to produce symmetrical effects, I should demur somewhat to the strictness, the hard-and-fast line, of that statement. In the first place, I accept entirely Sir William Gull's phrase as to syphilis being a flesh-and-blood disease. Amongst the class of diseases to which syphilis belongs, I do not know any pure blood-disease. Syphilis is more or less a blood-disease at different moments of its existence. At one part of its existence it is, of course, not only a flesh-disease, but also in a very intense and abundant degree a blood-disease; the contagium is plentiful in the blood; and that is a period in which all that comes from the body is infective: in which the sperm is infective, in which the secretion of any ulcer would be infective, in which the catarrh of the uterus and vagina would be infective, in which vaccine lymph taken from a child would be infective, and so forth. But, I apprehend, there is no abrupt line to be drawn between that condition and the condition of a patient with tertiary syphilis. The quantity of contagium in the blood gradually diminishes, and the power of it also gradually diminishes. As Sir William Gull capitally expressed it, there is here the strong contrast of the acute fevers. This is a disease of indefinite duration. As the quantity of contagium in the blood diminishes, a time comes, no doubt, in which it is a chance (but this is a question only of quantity) whether a particular secretion will contain contagium or not. In regard to the early stages of syphilis,

it is a tolerable certainty, and often I suppose an almost absolute certainty, that the patient will have syphilitic offspring. As he goes on, it is no longer a certainty; it is a chance; but that, I apprehend, is a question of quantity. He may have some syphilitic and some non-syphilitic children, but the successive children which are born syphilitic will as time passes be less intensely syphilitic. The probability of his infecting his wife diminishes even more rapidly. But in neither respect is there any abrupt line of difference; there are not two distinct stages of diseases. As regards the gummata, there again is a point where I would raise question as to the hard-and-fast line that Mr. Hutchinson, in some of his propositions, seemed to draw. He seems to speak of gummata not as directly syphilitic phenomena, but as new phenomena arising in parts which the syphilis, in its secondary stage, has affected: as sequelæ of syphilis, but with syphilis no longer effective in them. Well, I do not understand the pathological possibility of (in that sense) an active sequela; I understand a passive sequela, I understand a cicatrix; but when anybody talks of a morbid *process* as the sequela of a given cause, that means that the cause is still operative. In so definite a process (a process quite peculiar) as the growth of gummata, I cannot conceive the "sequela" otherwise than as representing the continued operation of the original cause. I do not differ from the positions which Mr. Hutchinson has taken in his paper, if the phrases are a little modified; if, instead of calling upon us for yes or no, he allows us to answer "more or less." I agree that, in proportion as secondary syphilis ceases to be an infective disease, the symptoms which it produces are likely to be unevenly distributed on the two sides of the body, as was explained by Dr. Moxon at the last meeting of the Society; but I do not regard that as a very important fact. I agree that, in proportion as one arrives at the period in which gummata develop themselves, in proportion as one gets the tumour-producing power of syphilis exhibited, one has evidence of a condition of the poison, probably a weakening of it, in which it is not likely to affect offspring. But, I confess, I do not think it would be safe in practice to take these as absolute propositions, and to say that, because a man has gummata, that man will not transmit syphilis to his offspring, or that, because a man's syphilis is unsymmetrical, he will not give syphilis to his wife. It would not be safe to take either of those propositions. I think Mr. Hutchinson's propositions may

be taken as approximations, but not as mathematically exact propositions. As regards a matter that has been adverted to this evening by Mr. Smith, the hereditariness of syphilis as compared with the hereditariness of gout, I think the two sorts of hereditariness are quite different. The hereditariness of gout is like the hereditariness of a Roman nose; it is part of a family likeness; the particular chemical type, the particular mode and rate of the chemical change of tissues, passes from the father to the son as the shape of features passes. But, with regard to the transmission of syphilis, the hereditariness is of a different kind; it is more like that which one has in comparative pathology, for instance, in some of the silkworm diseases; it is like the passage of a parasite in the ovum from one generation to another. It is not the case of the law of development inherited from one to the other, but the case of a material something passing from one to the other. Regarding the syphilitic contagium, in the present state of knowledge, it is no great assumption to suppose that it is an organic something, a growing something, a multiplying something; you have that something passing from the parent to the ovum and vitiating the development of the ovum. A question of much interest raised in Mr. Hutchinson's paper is as to the power of syphilis to modify other diseases, and particularly as to its convertibility into serofula. A consideration which I think an edifying one in relation to this question is, that the diseases into which syphilis is most accused of transforming itself are not the diseases which are most definite. The Pathological Society, with its accurate spirit of investigation, has not yet, I believe, succeeded in defining serofula. Tubercle we have got to know something definite about; but serofula, as a disease to be distinguished from tubercle, is but an unknown quantity for comparison. Be that, however, as it may, I have no knowledge or experience of syphilis either producing any other disease than itself, or being capable of modifying other diseases, though, of course, it may mix its phenomena with theirs. Here, too, I would qualify a remark made just now by Sir William Gull as to the variety of effects that syphilis produces in different subjects. Variety of degree—yes; but variety of effect—I venture to question. Syphilis is as definite a disease, I believe, as any disease known to our nosology. Although you have in different individuals great differences in intensity of result, I do not think you have any other great difference. That sort of difference of result you get in relation to all infective

diseases. Scarlatina, smallpox, measles—any one of these diseases may, as regards degree, show itself as differently as can be in one family from what it shows itself in another. And so, if you take a hundred cases of syphilis, no doubt you get very great differences of result; but those, I submit, are mainly differences of degree, and not differences that admit of being described as nosological varieties. As to the question raised by Mr. Hutchinson, whether gummata are deferred manifestations of a change of tissue affected in the second stage of the disease, I have expressed some hesitation which I would beg leave to repeat. It seems to me as more probable, looking at the whole history of syphilis, that gummata result from the weak action of a contagium which has been present in the body from the first, than that the tissue has all along been changed. The contagium may possibly have been there from the first, but, at any rate, has become weakened. As we see other evidence of the contagium weakening in the body affected, as we see it in relation to hereditary transmission, so here, where the contagium has been lingering in the tissue or in the body for a number of years, and presently produces its result, we should expect the result to be of low intensity; and it is only in accordance with what one knows in pathology that the irritant (if one may call it so) of this contagium, the irritant which in its stronger dose would produce an inflammation, would produce necrosis of tissue, in this late stage produces only hypertrophic phenomena, eventuates in tumour-formation. But if this question is to be well examined, it must be examined not from theory, but by fact. The aim should be to examine as many cases as one can get of secondary syphilis; and though it is very seldom that one can get for examination the dead bodies of adults with secondary syphilis, the opportunities of examining children who die with congenital syphilis, which is secondary syphilis, are innumerable, and they might be advantageously utilised for the Society. Examination of their tissues and reports on them to the Society would be of great value. Then, as regards the ulterior effects of syphilis, very important knowledge may be contributed in the matter of family history; and here almost every member of the Society, particularly those who are engaged in family practice, have opportunities of giving great assistance. Any one who has for long periods of time whole families under his observation is able to trace the true outcome of syphilis, and to see as a matter of fact whether other given diseases are less or more frequently produced—tubercular

diseases for instance, and bone-diseases, in the offspring of syphilitic families than in others. As to whether syphilis itself ever appears in more than the next generation, I may say that I am myself aware of one case where very strong suspicion was felt that syphilis had so manifested itself; that is, where a lady, whose father had died of tertiary syphilis, herself had a first child that was judged to be syphilitic, and had in her own person some symptoms which were ascribed to the same poison, there being not the smallest possibility of syphilis, as far as could be ascertained, coming to her in any other way than by that of descent. One would of course not venture to form an opinion on a single case, but these are matters in which very exact observation requires to be made, and in which very valuable material could be contributed by those who would keep accurate records of family history.

Sir WILLIAM GULL.—Let me offer a word in explanation with regard to what Mr. Simon has said as to the difference of effects or varieties of effects. What I wished to convey to the Society was this:—I said I thought that we had made a great gain since we had determined that there was one syphilitic poison producing a variety of effects according to the constitution. One person, for instance, has roseola, another has a scaly rash, another a papular rash, another a pustular rash. These are varieties of effects that were supposed to depend upon varieties of poison, and I wished to assert that we had made a great advance in believing that the variety of effects was due to the variety of people affected, and not to the original poison.

Mr. THOMAS SMITH.—Permit me to say that Mr. Simon is mistaken in supposing that I compared gout to syphilis; I endeavoured to contrast them.

Mr. SIMON.—Quite so.

Mr. JOHN WOOD.—Since these debates were first begun I have learnt a great deal. First, I have learnt the presence among us of some very old opinions which I am astonished to see crop up again. The old debates between humoralists and the solidists, one would have thought, belonged to the legendary history of the profession, but the discussion seems still to prevail. I am glad that most of the speakers, in fact, are agreed in the opinion that syphilis and other diseases affect the constitution, affect the whole body, the blood, the lymph,

and the solids. The definition which Sir William Gull has given us, like those which he so frequently gives, is a happy compendium; the term flesh-and-blood disease is certainly far better than the term blood-disease, though it scarcely covers the whole ground. It seems that this idea has also resulted in another phenomenon, which I am surprised to find is yet existent. There are persons still amongst us who believe in the multiplicity of poisons; at any rate, in there being two or three kinds of venereal poisons. I had brought myself to believe that the poisons of syphilis, the manifestations, whether primary, secondary, or tertiary, were simply the resultant between the virulency and the amount of the dose of poison originally imbibed, and the opposition of the system to the admission of the poison. In the case of a primary sore it is the admission of the poison into the blood primarily producing, in one case, indurated chancre, and, in another, soft chancre, and so on. In the case of secondary infection, where the disease passed from the blood into the solid tissues, the skin, for example, or the mucous membrane, we have another compromise or resultant between the resistance of the healthy tissues and the aggressive influence of the poison. We see the same in the infliction of wounds and various injuries which are more purely surgical. We see, in some instances, an amount of solid fibrinous material thrown up as if a foreign body in the tissues; in other instances it softens down into suppuration. You have here the difference between a hard and a soft chancre, or you have potentially the difference between a dry rash and an eruption results in the formation of fluid matters. I will now pass on to the point of the tertiary symptoms being sequelæ, or a continuation of the action of the syphilitic poison. I am decidedly of the same opinion as that expressed by Sir William Gull and Mr. Simon, that there can be no definite line of distinction drawn between these stages, and I think Mr. Hutchinson has done wrong in endeavouring to define such a line. He has fallen into the same error which many other syphilologists have fallen into in endeavouring to make a distinction between different kinds of poisons. So far as I understood Mr. Hutchinson, he seemed to agree with Dr. Wilks that all tertiary lesions were laid down during the time of the secondary fever, and that afterwards they simply followed the laws of ordinary growth. If this be the case, it seems to me that Mr. Hutchinson laid a little too much stress upon the symmetry of secondary diseases and the asymmetry of tertiary lesions. If the

sequelæ of simple growths be foundations laid down only during the secondary fever, we must also acknowledge that a patient while labouring under tertiary symptoms will not beget a syphilitic child, and will not be able to communicate the disease to another. Although Mr. Hutchinson and other speakers have alluded to the case of a woman in childbirth having scarlet fever, and the child being born covered with scarlet fever eruption—although this may be a transmission through the germ of scarlatina poison, yet, on the other hand, it may be simply a case of infection by contiguity. There is one point which has not been alluded to in the debate. Is it true or not that the races of men are becoming syphilised? Is syphilis milder in its manifestations than it used to be when it was first introduced among the European races? If this be not true, why do we not see the dreadful cases that were first seen on the coast of Africa? It is manifest that they cannot have been entirely due to the abuse or non-abuse of mercury. If syphilis be capable of effecting such a change in the races of mankind so as to render its ravages less virulent, it is quite clear that it will come under the classification of diseases like gout and rheumatism, especially the latter, which in many of its features has a strong resemblance to syphilis. I think this is an interesting point that ought not to be entirely overlooked in the discussion on this subject.

Dr. ROBINSON.—As regards the nature of syphilis, its unity or duality, I venture to think that the general opinion among army surgeons is in favour of the unity of the disease. The opinion on this point, expressed by Mr. Hutchinson and Sir James Paget, are entirely in accordance with my own. The disease, as it appears to me, depends upon certain conditions of the constitution, on temperament, upon what we may perhaps call the receptive condition of the patient at the time, and further upon the ablutions, if any, observed. I have always considered such circumstances as these as modifying the action of the poison in the system. Many years ago, in a detachment of the Guards in one of the parks, phagedæna occurred in a number of the men. In that case I was impressed with the idea that there are cases in which the virulence of the poison overrides constitutional infirmities. I remember also, in Limerick, a case in which it seemed to act in like manner. There was a perfect epidemic of syphilis there, all the cases being of one type, soft chancre associated with a peculiar indolent bubo. The

hospital was filled with that class of cases. It seems to me, therefore, that there are cases in which the usual mode of procedure of the disease is modified. With regard to hereditary syphilis, I think that the prevalence of hereditary syphilis exists to a much greater extent in private life than in the army. Why this should be the case is, I think, tolerably evident. In private life the condition of the patient is comparatively little known. In the army you have a considerable knowledge of the patient for a long period. Another point is that the sequelæ of syphilitic disease in the army are much less apparent. I should strongly hold to the view that syphilis in regard to its hereditary character is materially modified and reduced to a minimum by an early and judicious use of mercury. The last point to which I will refer is the connection between syphilis and phthisis. I presume that it is notorious to all that in the Household Brigade syphilis is very prevalent, and that phthisis is also prevalent. In a batch of seven or eight men that passed through my hands yesterday, who were discharged with phthisis, four had been under treatment for syphilis. I have felt that the connection between the two diseases is a remarkably close one. Were it not for the existence of several other equally potent causes, such as drink, exposure at night, and the use of a tunic that exercises an injurious effect upon the circulatory system, I should be disposed to think that syphilis was a more immediately exciting cause of phthisis; but it is impossible to dis sever one of these causes from the others.

MR. VENNING.—I am anxious to derive as much practical information as I can from this debate, and therefore I would ask Mr. Hutchinson if, in his reply, he will give me a few more details with regard to one paragraph in his paper.

I allude to that where he speaks of the theory of dualism, in which he makes the following statement:—"I think we may say of dualism that it is dead, and that the far simpler creed which attributes the soft chancre to contagion with inflammatory products, produced by syphilis, but not as a rule containing its germs, is the one which now attains general acceptance. We have, then, in syphilis but one malady and one virus."

Now, it is with regard to this "far simpler creed" that I seek for further information. I do not quite understand how a sore can be said to be attributed to contagion with inflammatory products

depending upon syphilis, and yet not containing its germs. Mr. Hutchinson's theory would place that form of sore which has been called by syphilographers the "local contagion sore" under the heading of a common inflammatory sore, for to speak of it as depending upon syphilis, and yet being devoid of its germ, would be like speaking of the semen of the body without the spermatozoa. But surely in this soft chancre we have something more to deal with than a common inflammatory sore. The definite course it runs, the fact that it is always attended with loss of substance, the inflammation of the lymphatic glands running on to suppuration, which so frequently accompanied it, and the fact that the same kind of sore can be produced by pus taken from the interior of the gland so affected, which cannot (in the earlier stage) be accomplished with that taken from the structure surrounding the gland, in addition to its perfect inoculability, and that through a series of inoculations, all tend to show that we have something to deal with of a different nature to a common inflammatory sore, and acknowledged to be different from the true infecting chancre, with all its sequelæ, or that third form of sore which occasionally occurs in persons who have been previously infected with syphilis.

Dr. FARQUHARSON.—I am glad that Mr. Venning has broached this subject; because, although Mr. Hutchinson has told us that dualism is dead, still I venture to think that something may be said for it; or perhaps I may say that dualism never had reason to be considered alive at all. I do not think that any one ever seriously supposed that there were two classes of venereal poison running their course side by side in the human constitution. I see no great reason why we should not consider that the soft sore may be an independent pathological unity, which may, without having any power of affecting the system, be able to carry on its operations in the system in local forms. If we cannot allow that there are two sores of this sort, I think that our difficulties are much increased. Formerly we could tell our patient, with a certain amount of security, that he would or would not suffer from secondary results. Now, however, it is impossible for us to tell him whether in future, after suffering with a soft sore, he will or will not be seized upon by the dreadful ravages of syphilitic disease. With regard to the analogy of the exanthemata, I think we are bound to take advantage of the difficulties which Mr. Hutchinson has himself raised in this matter. I think it

is not by any means necessary that the hard sore should produce secondary symptoms at all. Secondly, we have numerous irregularities in the development of secondaries. We cannot say beforehand what form any one will have. I think Mr. Hutchinson's explanation with regard to the effect of mercury as disarranging the evolution of syphilis, cannot in practice hold good. My experience is that mercury, when administered carefully and systematically, has the effect of rendering syphilis more regular. One point of agreement with Mr. Hutchinson's views regarding the likeness between syphilis and the exanthemata is, that very frequently there are much milder tertiaries after severe secondaries. There is another point, as to the lines of the tertiary disease being laid in the secondary. I can only bring forward one proof of that; that is a case that I brought before the Clinical Society last year, or rather a class of cases in which a certain form of hæmoptysis comes on in relation to secondary syphilis, which, if not checked by mercurial treatment, is apt to run into a tertiary formation.

Dr. GREENFIELD.—If I venture, sir, to make a few remarks on this important subject, it is because I think that hardly enough stress has been laid on the knowledge to be gained from morbid anatomy in the course of the discussion. It is, I think, to be regretted that, beyond some slight allusion to certain points, there has been but little reference to this side of the question, and that so little light has been thrown on the histology of syphilitic newgrowths. Certain questions have been raised which can only be solved by an appeal to the data of the *post-mortem* room, such, for example, as the question of the occurrence of gummata in the secondary period of the disease. But there has, I think, been too great a tendency to speak of the gumma as the sole and characteristic anatomical product of syphilis. That it is so in a high degree is beyond question, but it would be to ignore and reject a very large part of our knowledge of the effects of syphilis if we were to accept the term in its common use, and exclude from it those infiltrating forms of syphilitic new growth which Dr. Wilks has done so much to establish as truly syphilitic in their origin.

But it is evident that some part at least of the ground gained in our knowledge of syphilis during the past ten or fifteen years, on which Dr. Wilks has justly congratulated the Society, is in danger of being lost. For example, Dr. Fagge has expressed a

doubt as to the existence of syphilitic infiltration of the lung, in which Dr. Wilks, at any rate, believes. Mr. Hutchinson has spoken of the gumma as the typical product of tertiary syphilis, but he seems to look upon it as solely a form of inflammatory growth characterised by its hardness. I have no doubt that Mr. Hutchinson would employ the term in its widest sense, and would include under it the most minute nodule, and even an infiltration. But to a very large number of persons the term gumma is a synonym of a more or less caseous product; its essence is decay; it is nothing but a mass of small cells, aggregated together and rapidly undergoing fatty degeneration, and either becoming encapsuled in fibrous tissue as an effete product, or capable of rapid absorption. In this sense the gumma may be recognised in a large number of cases by its naked-eye characters in certain stages; but when cretaceous or even caseous, or when we have nothing left but a fibrous scar, we can only judge either by a knowledge of the history or by its locality and distribution that it is of syphilitic origin; so that, as Dr. Wilks long ago showed, we really depend on other features than the external appearance for our recognition of the cause, and it is a question of probability rather than of certainty.

When we come to inquire what are the microscopic characters of gummata, we are again met by the widest diversity of opinion, both as to what is actually seen and as to the relation which the growth bears to inflammation and to other morbid growths, and how far the microscope can aid us in distinguishing them. I will not detain the Society by describing the various views held by distinguished pathologists on this subject. But as regards the distinctiveness of these characters, even Dr. Wilks has said that, though he has from time to time regarded certain features as specific, he has been subsequently led to abandon them.

There are many reasons why it is very desirable that we should ascertain, if possible, what are the characteristics of syphilitic new growth, especially in its earlier stages. An immense number of lesions of various organs are ascribed, and no doubt with good reason, to the syphilitic poison. There is scarcely an organ or tissue in the body which is not, in some form or other, liable to affection in course of the disease. Quite recently the question of the relation of syphilis to the production of aneurysm has been hotly discussed, and is still undecided. Syphilitic disease of the smaller arteries was long ago described by Dr. Wilks, but has recently been

much more fully investigated. Again, it is well known that thrombosis is very apt to occur in syphilised subjects, but from what cause has been long doubtful. These and many other points indicate the desirability of extending our knowledge of syphilitic disease to the minuter elements, and not confining our attention to those changes which are visible with the naked eye. Mr. Hutchinson has raised the question of the occurrence of nervous affections in infantile syphilis; but in order to decide this it is absolutely needful that we should be able to recognise the earliest changes in the nervous centres before the growth of actual gummata. The mere fact of the occurrence of nervous disorder in the course of syphilis is of little value in proving causation.

Even if we consider only gummata, it is evident that what is most important is to trace the course of their development, for at some epoch of their existence they must be minute and recognisable only by the microscope. As often seen, syphilitic gummata are but the extinct craters of volcanoes, perhaps with a few smouldering ashes, but mainly distinguishable by the scorïæ and lava in the form of puckered fibrous cicatrices.

There are, undoubtedly, certain external features by which gummata are distinguished from other morbid products. Now, if it be allowed that these do not depend on the nature of their constituent elements, they must depend either on their arrangement or on their relations to other tissues; that is, just as a building gains its distinctive appearance from the architect's plan rather than from the bricks and mortar. In order, therefore, to ascertain the cause of its special characters, we must study the course of its origin and development, to see whether they present any peculiarities.

I have examined microscopically a large number of organs affected with syphilis, including gummata from various organs, at all stages, and infiltrating growths, as well as diseased arteries, with the idea of throwing some light on this question. And if I venture briefly to state what, so far, have been the points which have seemed to me the most important, I do not in any way pretend to have arrived at the solution of the problem, but simply to invite the observation and investigation of others. It is impossible to describe minutely here the various changes. I shall only briefly sketch them, trusting to a future opportunity to bring forward the specimens on which the statements are grounded.

During the period of their active growth and development, before

they have attained any size or undergone degenerative changes, gummata appear to consist in a measure of a sort of low form of lymphoid tissue, highly vascular, which is composed mainly of rounded or ovoid cells with very distinct nuclei and nucleoli, and a distinct nucleated stroma. If one examines the earliest form of infiltration, *e.g.* in the brain, it is found to consist of a growth of similar nature, extending mainly along the perivascular sheath of minute arterioles, not, however, forming nodular masses, as in the case of tubercle. By the increase and coalescence of these small tumours are formed. Very rapidly, however, there seems to occur thrombosis of the vessels supplying the new growth. This, so far as I can make out, is due to a thickening of the walls of these vessels, partly by growth in the outer coat, but also apparently by growth of the inner coat, which seems in part due to proliferation of their endothelium. This proliferation and consequent thickening of the inner coat can be readily seen in the small arteries of the pia mater surrounding a gumma in the brain, some very characteristic sections of which have been prepared for me by my friend Dr. Saundby. The change in these vessels is identical with that described by Heubner in his recent monograph, as affecting the cerebral arteries, independently of gummata.¹ But it can also be seen in the minuter arteries, not only of the brain, but also of the kidneys. It is, I believe, one of the most important points of distinction of syphilitic from other morbid growths, and accounts for the early necrobiosis of the newly formed tissue. Very speedily, however, either by a further growth of the small round cells, or from the irritation of their presence, a growth of embryonic connective tissue in and around the earlier nodules occurs, this tissue being characterised by the variety and luxuriance of its cells (as Virchow has observed) and by its high degree of vascularity. This growth takes place also along the walls of the occluded vessels, which become converted into fibrous bands containing an immense number of smaller vessels. According to Heubner, a single new vessel may thus be formed in the interior of the old one; but I have only been able to see the formation of a large number of smaller ones. The partial caseation and yellow colour of the gumma appear to be due to the earlier or necrobiotic changes; the firmness, smooth section, and persistence, to the later growth of a highly vascular though dense fibroid tissue.

¹ 'Die Luetische Erkrankung der Hirnarterien,' von Dr. O. Heubner. Leipzig, 1874.

The infiltrating growths do not appear to differ in any marked degree from gummata, save that in them the growth of embryonic tissue is in excess, the products of earlier decay being more rapidly absorbed, and leaving only slight traces here and there, the two processes going on side by side, and no actual tumour production occurring.

But even where gummata of some size exist some infiltration is also found, and in many cases the gummata gradually become more or less absorbed by the vessels of the fibrous bands running into their interior. I am convinced that there is no distinction between the elementary composition of infiltrating syphilides and that of gummata, and that we ought to speak simply of syphilitic new growth whatever its form.

In its later periods the growth becomes gradually converted into a more or less complete fibrous cicatrix, retaining, however, for a very long period traces of its cellular origin, and always the high degree of vascularity and the peculiar arrangement of its vessels due to their mode of formation. From a comparison of syphilitic cicatrices from various organs with other scar tissue, I am inclined to believe that the microscopic characters are in nearly all cases sufficient for their distinction.

I shall not endeavour to show the bearings of these points on the nature of the disease, nor to inquire what light is thrown upon it by the kind of tissue implicated, questions which will be better left until further observation and discussion with the aid of specimens are possible; but I would point out that the vascular and lymphatic systems seem to be especially involved. I have not entered upon the subject of affection of non-vascular tissues.

If it can be shown, as I believe it may, that the syphilitic new growth, in whatever form, is of the same ultimate composition, and follows the same course of growth; if, too, the existence of this growth is observed in cases dying at all periods of the disease, it seems to follow that it must be due to the continuous operation of one and the same cause throughout. And if it be allowed that this cause is the action of the syphilitic poison in the blood at one time, it must also be allowed to be so at another. The process is, to a certain extent, peculiar and specific, and proves peculiarity or specificity of the cause the more clearly because it is the process of growth, and not the tissue itself which constitutes the peculiarity of the product. And this, if true, would tend to prove that whether the morbid condition

be a modification of the blood or plasma, or an actual poison circulating in it, so long as any syphilitic growth is going on, the disease cannot be considered as terminated. Hence it is difficult for me to understand how gummata can be regarded by Mr. Hutchinson as *sequelæ*, unless the word is used merely in the sense of *results*. We are, I think, justified in assuming that the tertiary so-called gummata, as well as the infiltration, are simply growths, as Mr. Simon has said, which are due to the uniform action of the same cause, *i. e.* so long as there is syphilitic poison it goes on producing syphilitic growth.

There can be now no question as to the occurrence of actual gummata during the secondary stage, both in acquired and in infantile syphilis, the evidence being abundantly conclusive. I need only refer to Virchow and Wilks as bearing testimony to this fact. But infiltrating growths will, no doubt, be found with greater frequency at this period. It is so difficult to decide the limits of these arbitrary "stages," that it may often be a question as to which is to be selected for a given case. I have only had one opportunity of examining the body of a person who presented secondary lesions. It was that of a female, twenty-one years of age, who died from heart disease. There was a sore near the margin of the vulva, and some ulceration of the vagina; the tonsils presented recent extensive ulceration, and there was slight superficial ulceration of the pharynx. On the inner surface of the dura mater over the convexity of the hemispheres were a few scattered minute nodules, presenting all the appearances of minute gummata. Unfortunately the brain was not examined microscopically. The precise date of infection could not be ascertained, but it was probably less than eighteen months.

If I may venture now to turn to the subject of the general pathology of the disease, I would suggest that the analogy of syphilis with the zymotic or acute specific fevers has been somewhat overstrained. Up to a certain point a very close analogy must be admitted, especially when the latter are inoculated. In both there is an inoculation or entrance of the poison at some point in the system, as *e. g.* in smallpox or vaccinia. Then follows a period of elaboration and development of the poison, probably either in the tissues of the part or in the lymphatic glands, constituting the incubation period. Then follows the sudden diffusion of the poison into the blood, probably either in the form of solid particles or organised elements derived from the lymphatic system, or of a poisonous fluid; this gives rise to a fever, the onset of which may be

marked by rigors, and which constitutes the exanthematous fever which occurs also in syphilis, though usually in a slighter degree; we have, too, within a short period the occurrence of a rash. From recent researches it seems probable that all these symptoms may be due to the diffusion of germs or particles into the blood, which give rise by embolism to the symptoms of fever, and also to the proliferations of tissues and minute congestions or extravasations, and the like, which we call "rashes" of various kinds. The symmetry of the rash seems to depend, as Dr. Moxon has put it, on the fact that the blood is an impartial carrier to all the organs and tissues. Thus far the analogy of syphilis with inoculated specific fevers is complete. But here the analogy appears to me to cease, and the true blood affection to begin. In the acute specific fevers the morbid process has reached its maximum at this point; for a short period other changes go on, due in part to the progress of the lesions thus set in action; but speedily the poison germ is either eliminated or becomes effete; after having multiplied or "seeded" at the expense of the organism, it is extruded, and nothing remains but its effects, which are "*specific*" only in a limited sense, that is to say, the weakness or inflammation tends to affect certain organs or tissues, but no specific or peculiar product is produced. With syphilis the case is different; from the period of the diffusion of the poison into the blood, changes occur in the blood and tissues of a peculiar and specific type, going on perhaps for years, and affecting the whole organism in a special manner. The analogy seems then to fail from this point, and we may fairly ask whether the early resemblance is not due merely to the similarity in the mode of entrance of the poison; in the one case the poison being as it were an epiphyte, which grows at the expense of the system, and is then (itself or its offspring) ejected; in the other the poison depending on a modification of the tissue elements themselves, and growing from them, is capable of indefinite multiplication, modifying the whole system, and bending it to its laws of growth, just as the elements of a morbid growth tend to produce their like by the infection of healthy tissues. Nor is there any real bar to the acceptance of this view in the fact that the contagiousness of syphilis seems to be limited to certain products of the secondary period. We need not seek far in vegetable physiology for cases in which there is a power of reproduction by seeding only at certain periods, and the plant can also be reproduced by grafting or budding at any time. The chancre and certain secondary secretions are, so to speak, the

fructification and seeding of the morbid process, but there is abundant evidence that the disease may be afterwards grafted or budded in other ways.

From this time, then, it seems to me that we must regard syphilis as truly a *blood* disease, modifying probably the blood-cells, and, through them the nutritive fluids. From this period we have a tendency to the production throughout the system of growths which follow a certain definite order and type, either as minute growths, infiltrations, or larger tumours, forming the class of symptoms known as tertiary.

In addition to these, however, the long course of infection of the blood gives rise to certain changes in the nutritive properties of that fluid, which may lead to the production of common irritative or inflammatory changes (such as are also caused by the poison of rheumatic fever and gout); hence probably arise some of the pains in the joints, affections of the serous membranes, and of the lining membranes of the heart and great vessels, such as peritonitis, endocarditis, &c. To similar causes it is highly probable that we must refer lardaceous degeneration of the viscera.

But beyond all these, in the long course of the affection of the lymphatic system and the blood there appears to be produced a degeneration of the nutritive processes, analogous to that observed in some so-called "constitutional" diseases, and which displays itself mainly by the imperfect nutrition and re-formation of certain organs or tissues. The changes of which I speak are more commonly seen in those cases where the disease has lasted for a long time. There are certain lesions almost peculiar to so-called hereditary syphilis which seem to be of this nature, such as the affection of the bones, the teeth, and the cornea. Some of these may occur at a long period after the occurrence of the true syphilitic symptoms. But I have been led to doubt whether some of the symptoms of so-called "hereditary" syphilis are not merely due to an heredity of a specific form of malnutrition or tendency to early decay of peculiar character, and not to the existence of actual syphilis in the offspring.

In the syphilitic parent we find that there is a great over-production and mal-production of connective tissue. Now, if Mr. Hutchinson's view be correct, viz., that syphilis is a specific fever, it would be an additional proof of that view, if the inherited portion of it were not the disease itself but something merely the *effect* of that disease. And may we not readily believe that this excessive malformation

of connective tissue in the parent may be exhibited in the offspring by a tendency to early degeneration or malformation of certain of the more highly developed or specialised forms of connective tissue, such as the cornea, teeth and bones; and that they are an expression of the exhaustion of the power of connective tissue formation.

I should like to ask Mr. Hutchinson whether there is evidence that in *all* cases of so-called hereditary syphilis—that is, the cases which present only notched and pegged teeth or interstitial keratitis—the children have always suffered from symptoms of true syphilis in infancy.

It may be urged that syphilis may exist during infancy, and that its existence may pass unnoticed, or a slight rash may be the only thing which is observed and this be ascribed to something else, or that where no history is attainable syphilis must be supposed to have existed. Now, if due to actual syphilis in the child itself, these lesions should be seen in cases of early vaccinal syphilis, or should be more likely to occur in them. Is this the case? The fact that a child grows to resemble its parent much more towards the time when these lesions show themselves must also be remembered. Again, a peculiar form of hyperostosis, usually considered peculiar to infantile syphilis, is in rare cases met with or closely simulated in very long standing-cases of acquired syphilis. This form of bone disease has been studied by many observers, but more especially by Valleix formerly and Parrot of late years.¹ This disease, common in hereditary syphilis, occurs in rare cases in long-standing acquired syphilis. I hope shortly to bring before the notice of the Society the case of a man who had been the subject of progressive syphilis for twenty-five years, and who presented the most advanced degree of syphilitic hepatitis I have ever seen. In this case there was a condition of the long bones precisely similar in all its characters to that described as peculiar to infantile syphilis, allowing of course for the differences between a developed and a growing bone. The condition too was such as I have never seen in any other form of bone disease. Now, this change in the bones, so slowly produced in this case occurs, from the earliest date in the child, and may not improbably be merely due to an inherited tendency showing itself in their development.

¹ For a concise description of the changes found in the bones in the several stages of the affection, I may refer to a recent paper by M. Parrot, published since the above remarks were made, in the '*Archives de Physiologie Normale et Pathologique*,' No. 2, 1876, p. 133.

The fact that actual symptoms of syphilis exist in some of these cases does not at all negative this hypothesis. The child under such circumstances may and probably will have both heredity of peculiar malnutrition *and* communicated syphilis, and the interstitial keratitis or pegged and notched teeth may yet be due to the former alone. As presenting some analogy, and showing how a general constitutional influence acting on the parent during gestation may affect the development of the child *after* birth, I may mention what occurs in cretinism. It is well known that goîtrous parents living in a locality where goître is endemic may during their residence in that locality beget children who become cretins, but if they remove from the locality the children subsequently procreated do not become cretins. Now it must be remembered that cretinism is a condition which develops after birth, and that it depends, according to Virchow, on an imperfect development and early ossification of certain portions of the base of the skull.

Hence, whilst I do not for a moment question the connection of these lesions with heredity from a syphilitic parent or parents, I think the above hypothesis tenable to explain their mode of production. Moreover, many cases are known in which these persons acquire syphilis; this, though not a proof, is an *à priori* argument that they have not had true syphilis.

And here I must remark that I am led to question whether infantile syphilis is really *hereditary* in the strict sense of the term. There seem to me to be some weak points in the argument. It is said—well, there are cases on record, in which careful minute investigation was made, and no symptom of syphilis, past or present, was to be discovered in the mother. But as I have said, and Mr. Hutchinson would I think allow, this is no proof that the mother has not had it. Cases are frequent where syphilis shows itself by tertiary symptoms long after infection, where there is no proof of secondaries having occurred; and of gummata being found in the body after death where no symptoms were observed during life. In the cases of one single coition, of which I believe some are on record, the evidence again is imperfect, and it is scarcely credible that frequent connections should occur during a long course of time, and the mother remain uninfected, and yet the child be infected by perhaps a single spermatozoon.

The period at which the specific rash and other symptoms usually show themselves in the child is very nearly the same as that at which

they occur after inoculation in the adult, and the disease may not improbably be communicated from the parent at the time of separation of the placenta, as has been suggested by Dr. Cory, when for a time there is possibility of direct communication of the blood of mother and child, or at birth. But on the other hand there is the fact that, as Mr. Hutchinson has said, the poison seems to lie dormant in some cases. As an illustration of this I may mention a case now under my care.

A child was vaccinated at the age of $3\frac{1}{2}$ months; previously to vaccination the skin was perfectly clear and free from eruption. At about six or seven weeks from vaccination a "measly" rash, as the mother called it, suddenly appeared, and increasing spread all over the body. Vaccination was performed on January 1st, the child having been born on September 12th. I saw the child on March 8th; it was then covered with a rash, mainly of papular character, large coppery blotches, some large ulcerated papules, &c.; it was, in fact, a typical case of syphilitic eruption. Since then some mucous tubercles have developed.

The vaccination marks were very distinct, one scab not yet being separated. But there was no induration whatever of the scars, and although the glands in the right axilla, on which arm the vaccination was done, were somewhat enlarged, they were equally enlarged and indurated in the left axilla. Moreover, on further inquiry I found that the mother had had three miscarriages at a period of about six months, and that the child had snuffled from the birth. Hence it seemed more probable that this case, at first sight a typical case of vaccinal syphilis, was really one of congenital nature, and that the irritation of vaccination had merely served to evoke the dormant poison. I mention this case as being in some measure confirmatory of Mr. Hutchinson's views on the cause of appearance of the rash at the particular time when it is usually observed.

In conclusion, I cannot but express the hope that this discussion will serve to direct attention to some of these difficult points and to lead to their elucidation, and this must serve as my apology for bringing these considerations before the Society.

Dr. SEPTIMUS GIBBON.—With reference, sir, to the important remark of Mr. Simon, that he has witnessed a case of syphilis in the third generation, I wish he had specified the lesions observed, because I entertain serious doubts as to the truth of Mr. Hutchinson's doc-

trine that there is no relation between syphilis and tubercular disease. It has been my lot to meet with cases of tubercular diseases whose origin I could attribute to no other cause than syphilis in the paternal grandfather. They occurred in Hebrews, who are notoriously exempt from scrofulous and tuberculous taint. In confirmation of this fact I may, perhaps, be allowed to mention that during the last twenty-five years I have attended a large number of Hebrews, both as hospital and private patients, and have never met with a single case of pulmonary consumption amongst them, and a former colleague at the London Hospital, who had a large *clienitèle* amongst this favoured people, told me that for forty years he had only met with two cases. The medical officer of one of their large schools has remarked that their children do not die in anything like the ratio of Gentile children; and in the district of Whitechapel the Medical Officer of Health has reported that on the north side of the High Street, occupied by Jews, the average death-rate is 20 per 1000 inhabitants, whilst on the south side, occupied by English and Irish, it is 43 per 1000. Side by side with this superior vitality and marked exemption from scrofula we have the facts that their females do not practise prostitution, and that circumcision, as conclusively proved by Mr. Hutchinson, confers on their males an immunity from syphilis four times greater than that enjoyed by Christians. In these broad facts there appears to me to be some presumptive evidence that syphilis is the parent of scrofula in, it may be, remote generations.

When Jews have syphilis they suffer severely from primary and secondary symptoms, and little, if at all, from the tertiary. I can point to patients who are the pictures of health, and would, I fancy, defy Sir William Gull's nasal test, as they certainly do the anæsthesia test relied on by some French practitioners, and yet they have begotten syphilitic and unhealthy children.

For many years I have attended a family where the grandfather admitted that he had syphilis; his children are anæmic and constantly under treatment for dyspepsia, rheumatic pains, and slight ailments, but have had nothing of a tubercular form since I have known them. I am unable to say whether they exhibited the symptoms of inherited syphilis in infancy; they certainly show none of the physiognomical signs of the inherited disease. But the grandchildren in two families have had scrofula in various and unmistakable forms; one child has had scrofulous abscess of the testicle,

another psoas abscess, another scrofulous disease of the bones of the arm, and others ulcerations and abscesses about the neck and ears. All have contraction of the upper jaw. On investigating the family history of these cases I failed to detect any scrofula taint on either the father's or mother's side. Is it not, therefore, a fair inference, from such cases as these, to conclude that syphilis is *one* of the sources of scrofula? I readily admit that there are other causes, such as want of bodily exercise and residence in damp and dark places.

As to the classification of syphilis with the exanthematous fevers, I cannot agree with Mr. Hutchinson, and, in addition to the numerous points that have been mentioned, I would point out that the analogy does not hold good in the matter of treatment. Mercury modifies and controls the symptoms and duration of syphilis; some surgeons even hold that it is a specific cure for the disease, and I would ask Mr. Hutchinson whether there is any specific or any agent that will modify or control the symptoms of measles, smallpox, or scarlet fever? Did he ever see these diseases or any exanthem either cured, cut short, or modified by treatment?

Although mercury and iodide of potash temporarily remove the blotches, the iritis, and "gummata" of syphilis, I do not regard them as specifics, because in the great majority of cases when the remedy is left off these symptoms return, and often the last state of the patient is worse than the first; certainly, the worst cases of rupial sores, nodes, and necroses of bone I have witnessed have been in patients treated by mercury. I believe Mr. Hutchinson based his doctrine of mercury being a *specific* for syphilis on a series of vaccinal syphilis in children and young adults, because under its use the symptoms passed off; but, as Dr. Drysdale has truly observed, the symptoms in such cases would almost as readily have passed away of themselves without the use of mercury. I am inclined, sir, to think that we shall best discover an antidote, and get at the true nature of this wide-spread and fearful disease by a thorough and minute investigation of its action on the blood in the secondary stage. If the Society would undertake such a careful investigation it might, perhaps, decide whether it is a poison or a fermentative parasite, as well as its relation to the exanthemata and other points so ably dwelt upon in this discussion. The action of the poison is apparently to break up the red globules of the blood, and to allow the hæmoglobulin to escape and form blotches on the skin.

Hence, in contradistinction to the *medical*, it might be called a *surgical* exanthema. The best observers state that the red globules are not again restored in anything like their former number; the consequence is that the supply of oxygen to the tissues is very seriously diminished, and the effects on nutrition and ordinary health are most disastrous. Is not this large withdrawal of oxygen from the blood and tissues sufficient to explain the formation of nodes and gummata without the far-fetched hypothesis of Mr. Hutchinson and other speakers, that they are the direct product of the poison or regrowths of the parasite? I am disposed to think it is, for the reason that all nodes and "gummata," if treated moderately early by mercury, iodide of potash, or even peroxide of hydrogen, whose actions, so far as we know them, are simply to convey oxygen to the tissues, can be quickly removed, but if kept too long they are difficult of absorption and may induce other morbid changes.

Adjourned Discussion, April 4th.

THE PRESIDENT.—Perhaps before I ask Mr. Hutchinson to reply to the various observations that have been made, you will allow me to make a few observations which I would have done at our last meeting, had it not been that we were so late in our discussion, and I was unwilling to detain you. I hope Mr. Hutchinson, being present, will accept anything I say in a friendly spirit if we should differ, for I confess I have felt very much like a pupil sitting at the feet of a teacher in listening to all I have done from Mr. Hutchinson and others, having learnt much and being still anxious to learn more. Mr. Hutchinson says, "Are there any facts which favour the belief that syphilis continues to be a blood-disease, after the cessation of all tendency to produce symmetrical symptoms?" I think the discussion that has taken place has produced sufficient evidence to show that we are not agreed upon this point. When we have to deal with such a poison as syphilis, I think we must look at it entirely as a disease *per se*, as stated by Dr. Wilks, to whose observations, I think, everybody must have listened with very great interest and instruction. If we contrast syphilis with the various eruptive diseases attended by fever, we find that each of the latter runs its limited course, terminates at a stated period

without leaving any consequences, and is not influenced by any medicine or treatment. The disease terminates without leaving any sequelæ of a specific character, and to this I shall refer presently; but it leaves the patient in a condition which usually renders him free from any subsequent attack. As a general rule, he has no power of infecting his offspring with that disease, nor does he render that offspring free from its contagion conveyed to it by any other means. But with regard to syphilis, we see a very small poison introduced into the system, followed by an eruption, and followed by those consequences which are termed the sequelæ of that eruption—the tertiary condition. I agree with Dr. Wilks in thinking that the terms “primary, secondary, tertiary, and quaternary” really are of very little importance; they rather map out phases of the disease, than indicate anything very specific: and when we see the disease in its secondary condition evidenced merely by an eruption, we really should consider that that eruption is only one indication of a very large poison affecting the whole system; and how far we can take that secondary eruption as an evidence of a blood-disease, and that that blood-disease terminates when the eruption is no more symmetrical, but, disappearing, leaves behind it certain conditions which we term tertiary symptoms, does not appear to me at all clear. Mr. Hutchinson has stated that it would be rash to assert that it was impossible for tertiary syphilis to prove contagious, but at present we have no evidence which would support this affirmation. With regard to that, I should like very much to ask Mr. Hutchinson if he is satisfied that we should not get infection from inoculation of a healthy subject from a tertiary sore or tertiary discharge of matter from any person affected by tertiary syphilis; and unless we had these experiments largely carried out, I think we cannot safely arrive at any satisfactory conclusion. Mr. Hutchinson says the risk of contagion appears to cease long before the risk of hereditary transmission. I should like to ask him if he can give us any hint as to how he arrives at this conclusion, and if he can tell us when this tertiary condition ceases to be innocuous, and enables the father to beget a healthy child. That a person suffering from tertiary syphilis is capable of producing disease in his offspring we know to occur; and I mention the following case as illustrative of a point which I propose to follow out in asking Mr. Hutchinson a question. A patient was admitted into

St. George's Hospital with a syphilitic affection of the larynx. She was suddenly seized with great difficulty of breathing and impending suffocation, and I had to perform tracheotomy upon her at a moment's notice. The evening of the operation, she was delivered of an apparently healthy child. The next day the nurse drew my attention to a sore on the back of her left thigh, and that was a solitary unsymmetrical rupial sore ; so that, taking Mr. Hutchinson's description of the conditions of rupia, that when non-symmetrical it is of the tertiary form, there we had evidence of the tertiary condition of syphilis giving rise to what within three weeks proved to be a syphilitised child. The child within three weeks, although born healthy, was covered by an eruption about the nates, scrotum, and the lower part of the body. To carry on this point with regard to the condition of the blood-disease in the primary and the secondary or the tertiary stage, I wish to mention another case. A woman was admitted into the hospital with a hard smooth ulcer on the surface of the left breast. The base of the ulcer was extremely hard, and that went on gradually increasing slightly in size, and was attended by a slight copper-coloured eruption. I particularly went into her history, and it appeared that the woman had lost her own child, and had had another child sent to her to be nursed ; that child was covered by a secondary eruption, with a sore on its mouth, and had given rise to a chancre of an undoubted character on the nipple of this healthy woman ; and under a course of mercury she ultimately recovered. If, in the tertiary condition of syphilis, accepting Mr. Hutchinson's theory or interpretation, we have the disease ceasing to be a blood-disease, and becoming merely localised in the tissues, soft or hard, I ask how he explains that the tertiary disease gives rise to the secondary in the offspring, and the offspring that has secondary disease produces the primary sore on the breast of a healthy woman ? When is it, or how is it, that the tertiary disease ceases to be a blood-disease, and how is it that it merges by transmission into the primary condition of a blood-disease ? Then if the tertiary disease be not a blood-disease, but a local disease—I speak as a surgeon—I would ask him how it is that in plastic operations, attempted upon a patient suffering from tertiary syphilis, we do not get union—that the wound is affected by the condition of the patient ? A medical gentleman had been suffering from acute syphilis with gonorrhœa ; he had an abscess in his perinæum, which was rather boldly opened, and unfortunately a communication made between

the bladder and the rectum. He came to me two or three years afterwards with tertiary affection of the nose and the palate, in the greatest distress ; he had no command over the bladder, and begged me to operate upon him. Seeing his condition, and seeing how saturated he still was with the poison of syphilis, I begged him not to undergo an operation, for I was certain it would fail. However, he pressed me so hard that I at last consented, very much against my own judgment ; and operated upon him. He had considerable secondary hæmorrhage, and there was not the slightest attempt at union. I would ask any surgeon present if, with tertiary syphilis affecting the face and nose, he would not allow a considerable time to pass before he would attempt anything like a plastic operation, where union by first intention is so necessary for success ? These are the points that have occurred to me, as militating against the idea that you can separate the secondary from the tertiary conditions of syphilis. Or rather, is it not more important that we should consider the disease as a whole, and in its tertiary condition that it is running on perhaps to its end, mitigated by treatment, moderated by time, altered in circumstances by those daily changes that take place in all structures, and perhaps approaching that end, which by a little more treatment and by patience may, at any rate, place the patient in a condition in which he may be considered free ? But experience teaches us that, under adverse circumstances, mental distress, exposure, depression from treatment necessary for other illnesses, the disease may manifest itself again, and at any period of life. Many years ago, when in Canada, I was asked to see an officer between fifty and sixty years of age. He had two or three soft nodes on his skull ; he had been treated for a severe attack of bronchitis in the middle of a Canadian winter. In questioning him whether he had ever had syphilis, he seemed rather surprised, and reverted at once to his ensigncy in New South Wales, where he had contracted syphilis ; he had almost forgotten the circumstance. There must have been an interval of nearly forty years between his primary sore and its tertiary development ; and I looked upon that as the development of the tertiary symptoms the result of his deteriorated condition of health and the treatment which he had undergone. These are the points which appear to me to bear very strongly upon the fact that, as long as tertiary syphilis manifests itself, we should consider it syphilis, to be treated as syphilis under any other circumstances ; or, as Sir James Paget said,

with iodide of potassium for its passing cure, and mercury for its ultimate relief and eradication. With regard to the point that Mr. Hutchinson brought before us—and, I think, with very great ability—namely, how far syphilis is connected with scrofula, or how far it produces scrofula, I believe that there is no evidence that one can satisfactorily rely upon that syphilis is in any way the parent or the relation of scrofula; but I believe scrofula may very materially influence the severity of the syphilitic attack, just as other circumstances may do. I think this is a point of very considerable importance: we have climate, we have constitution, we have temperament, we have also the nature of the poison, to a certain extent, influencing the condition of the patient who is running through a course of syphilis. I have an interesting communication from Dr. Duka, who has seen a good deal of syphilis in India; he says, with reference to the manifestations of syphilis in India, “it seems that the virus of mixed races—that is to say, a European infected by a native woman—produces a more obstinate disease; the primary sore also being more liable to run into the phagedænic type than is the case otherwise. There is no doubt, however, that the natives are much disposed to the ravages of the disease.” As far as my experience goes—and I think very likely Mr. Hutchinson will confirm this—some of the most severe cases of tertiary syphilis I have seen have been the result of disease contracted in China and in India. I think it is necessary that I should refer to what Sir James Paget said with regard to the sequelæ of syphilis in any degree resembling the sequelæ of other fevers. Now, I cannot bring myself to agree with Sir James Paget in this, although I hope in most matters we should agree. I look upon the sequelæ of fevers—and I think I shall be supported in this view by many—very much as accidental. They are in no degree related to the fever itself, after whatever type of fever they may follow. Take the sequelæ of typhoid: I have generally considered the sequelæ of typhoid as a pyæmic condition rather than as anything due to the typhoid-poison; and I have seen these sequelæ symmetrical as well as non-symmetrical, two nodes occurring upon the tibia in more than one instance. And I think those sequelæ resemble in their character the sequelæ of lying-in women—phlegmasia dolens; or phlebitis observed in cases of foul abscesses in the lungs. Therefore, I do not think there is any analogy between the sequelæ of syphilis and the sequelæ of other fevers. It is a pity that we should confound

the two. I look upon the sequelæ of syphilis as syphilis, and upon the sequelæ of other fevers as purely accidental. I have ventured to introduce these few points as they have occurred to me; but I cannot sit down without congratulating the Society on the very able paper that we have received from Mr. Hutchinson, and congratulating Mr. Hutchinson upon the very fine chain of philosophical thought that runs through the whole of his communication. I think the Society is very much indebted to him, not only for the paper, but for the discussion that has followed it. I received a letter from a friend who was present at one or two meetings, and who has read the other portion of the discussion; he writes to say the only conclusion he could come to was that surgeons, as well as doctors, differ. Well, if we differ, gentlemen, in one or two points, it is merely a theoretical difference, I believe; and a difference which perhaps will not prove of any serious importance; for, after all, this question whether syphilis in its primary condition be a blood-disease, and in its tertiary condition is a disease of the tissues, is of no importance to us. We shall not differ with regard to the true pathology of syphilis, for the knowledge of which we owe so much to Dr. Wilks and others; nor shall we differ very much with regard to the treatment. Perhaps it is a little diverging from the point if I say here—and I think it will be acceptable to many of us—I do not think we shall differ in the view that perhaps this discussion may strengthen the hands of those who are moving for legislative enactment to suppress to a certain extent the evil of syphilis; or, at any rate, to modify the great amount of syphilis that exists amongst the lower classes. In saying this much, I will conclude merely with a hope that the light which was spoken of by Sir James Paget may, by the work of Hutchinson and others who have laboured so largely in the field of syphilis, be yet held aloft as a beacon to guide us, and steer us straight in the troubled sea of the complications of syphilis.

MR. JONATHAN HUTCHINSON.—I must begin first with an apology to you, Sir, and to the Society, that I was not present at the last two meetings; and I must say that it was from no intention on my own part, but from circumstances quite unavoidable. I have, however, very carefully studied the very accurate reports of the discussions in the journals, and I hope I shall not do any speaker

injustice from not having been present. I feel also that I have reason to congratulate the Society on the fact that my absence on the last occasion was the means of procuring for the Society the speeches of Sir William Gull, of Mr. Simon, and of Dr. Greenfield, all of which we should have been very sorry to have missed. I think I perhaps also ought to apologise for the fact that I am going to read my reply; but it is long, and I am sure, if I did not read it, I should occupy very much more of the Society's time, and probably to less purpose. With regard to the remarks which have just fallen from yourself, I am exceedingly obliged for them, because they have given me a sort of summary of the discussion, and expressed very concisely your views, and I dare say the views of a great many, as regards the chief points which have been raised. I am further very glad to say that I find you and I differ scarcely at all; it is only on the interpretation of one or two minor facts. As regards most of what you have said, I have dealt with that in the reply I am about to read. With reference to your remarks as to the inference from the fact that a surgeon would not like to operate upon a patient who was the subject of tertiary syphilis, believing that the wound would not heal—

The PRESIDENT.—A plastic operation.

Mr. HUTCHINSON.—Admitting the fact to the full extent you have stated, I should not allow that it implies any reason for believing that the blood was tainted.

It will, sir, I think, have been obvious to most that two principal ideas ran through the remarks which I had the honour of addressing to the Society in introducing this discussion. The first and most important was, that syphilis depends upon the introduction into the system of a living material which is capable of self-multiplication, which breeds in the blood and tissues, and which is destined to pass through various stages of development, and finally to die. My second idea was, that, in order to a correct clinical comprehension of all that follows on syphilitic poisoning, we must admit, in connection with the phenomena which rank as its direct results, certain less closely associated peculiarities of inflammation and of cell-growth. In this way, I tried to show that soft chancres might result from contagion with pus produced by syphilitic inflammation, but not actually containing the syphilitic sporules; that phagedæna in like manner might produce a contagious secretion, but not a syphilitic one, and that it might wholly escape the restraint

of the specifics for syphilis, and that a gumma might grow long after a patient had ceased to possess either in his blood or tissues the living virus of his original malady. These various events were, I suggested, to be reckoned rather as adventitious to the syphilitic fever than as true parts of it; and to the later forms of gumma, &c., known as tertiary symptoms, I thought the name *sequelæ* the most appropriate, meaning by that term that they came after the true syphilis was over, just as we know that as a sequel to war may come a famine, though the war itself is ended. It will, I think, be readily granted that the correct appreciation of the relationship of the soft sore, of phagedæna, and of tertiary symptoms, to syphilis itself is all important as regards our insight into the kinship of the latter to other more short-lived exanthems. If the tertiary symptoms are not, in a strict sense, a part of syphilis, and if in reality the latter be usually over in a year or two from its outset, we are able at once to realise much more easily the kinship referred to.

Now, in looking back on the very important criticisms which have fallen from the various speakers who have so ably sustained the debate which ends to-night, I find, on the whole, a very considerable amount of concurrence in the views expressed; by no means, however, without some expressions of misgiving and dissent. By one or two it has been hinted that there is no proof of the existence of a germinal poison as the cause of syphilis; others have thought that the analogy with specific fevers has been pushed too far; that the stages of syphilis are so irregular, that they can scarcely count as such at all, and that it is wiser to speak of the disease as one whole; whilst many have expressed doubts as to whether it be possible for any results to persist, unless the blood still continue tainted. To take the first of these objections, I have to reply to Mr. de Mérie—who reminded me that the germ-poison of syphilis had never been put under the microscope—that surely there are cases where deduction amounts almost to proof. With regret I admit that I have never seen with my outward eye the cryptogamic germ-poison of syphilis; but to my mind's eye it is as certainly present as if I had. Some one will see it some day, for it is beyond doubt that it must be there. I would even venture to suggest that it might be wise to anticipate discovery a little further, and to speak of this *contagium vivum* as the *syphilitic yeast*, so that we may force our minds to keep clearly in view the possible developments

of this theory both as regards the inherited and the acquired disease. When a better theory is forthcoming, we may lay this aside, together with the forms of phraseology which it has brought in ; but I cannot but note, as a remarkable result of this discussion, that, despite certain objections to this, there seems to be no other theory in the field. For the present, to abandon the germ or yeast hypothesis of syphilis would be to throw the subject back into confusion ; for it, as far as I can see, is the only clue which we possess to the orderly arrangement of our facts. Whether or not it will, when carefully developed in detail, be proved sufficient to explain all the facts, is the problem which confronts investigators. For myself, I may admit that the result of this debate has been to strengthen my belief that it is not probable that it will ever be supplanted. I have failed to see what those who suggest that the analogy with the exanthems has been pushed too far, have advanced in support of their criticism. I can only again express my conviction that the more the facts as to syphilis are examined, the more clearly will it appear that, when allowed to develop without interference by specifics, its stages are very fairly regular, whilst the more we know of the other exanthemata, the more willingly shall we admit that theirs are by no means so definite and precise as we are accustomed to assume. Whether or not it results from our habit of prescribing remedies from which no very definite effects are expected, I cannot tell ; but certainly it seems wonderfully difficult to persuade ourselves that mercury really does interfere with the development of syphilis, and that it is hopeless to attempt to get correct ideas of the natural course of the malady, if we investigate cases in which it has been used. Amongst the facts which chiefly impressed on my mind the lesson that syphilis, when let alone, is really a very orderly disease were the two now well-known series of cases in which, some years ago, I had the opportunity of examining a number of patients who had all at certain definite dates been made syphilitic by vaccination. In the first of these, about ten persons had indurated chancres on their arms at the same time, and due to the same cause. I do not think that any two of their sores varied more than a week in stage of progress from each other. The similarity in date of induration, &c., was most remarkable, and quite as definite as would have been the phenomena of vaccination or of smallpox in a like series of cases. In my second series, the stage of the disease was further advanced before the accident was dis-

covered ; and here, again, we found syphilis very regular, since case after case was sought out and found to be in almost exactly the same stage as its fellows.

If, however, mercury be given, the course of things is wholly altered, and it depends upon circumstances whether the exanthem stage be wholly prevented or only very much deferred. In a case in which mercury was begun early and continued for six months, I have known the rash, which would otherwise have come within six weeks, deferred till the end of the period named. Here I am obliged to interpolate that I did not exactly see Dr. Farquharson's meaning, when he said that mercury makes syphilis more regular. That it makes it milder is undoubted, and that it makes its stages, as a rule, much shorter is most undoubted ; but I feel sure that, in certain cases in which it fails to cure, it may greatly protract the malady by increasing the distances between its stages. The theoretical explanation of this is, that it probably prevents the development of the yeast in the blood ; and, if it do not wholly kill, it leaves it still able at some future time, when the antidote has been laid aside, to resume its growth. In connection with this theory, I wish to direct attention to a most important suggestion which fell from Dr. Broadbent, and which was especially welcome as a striking exception to the remark I have just made as to the general incredulity now prevalent as to positive therapeutics. Dr. Broadbent remarked that, now that it has been proved that mercury is an antidote for syphilis, and made probable that the syphilitic virus is the same in its nature as those which cause the exanthems, we ought to again make careful trials of that and other specifics for them. The same thought has often occurred to my own mind. If mercury can kill or retard the development of the yeast-plant of syphilis, it is very probable that it or the iodide, or some similar remedy, may do the same for the yeast of typhus, typhoid, or small-pox. We must not assume that this question has been set at rest by any trials which have as yet been made ; for they have not, I think, been carried out with sufficient care. It will be desirable to ascertain carefully, beginning at the very earliest possible period, whether the evolution of the disease, as tested by the temperature and other symptoms, can be modified ; whether the stages can be protracted ; and, this point having been set at rest, we must then inquire whether such retardation is, on the whole, for the good of the patient, and determine the clinical details to which it may be

necessary to attend. It is quite possible that the same remedy may be very useful in one mode of administration and very hurtful in another, and thus the statistics *en gros* of recoveries and deaths under mercurial treatment in typhoid, which have been collected hitherto, may have no close application.

The discussion which has taken place as to when syphilis ceases to be a blood-disease has been very important; but it has been in part based upon a misapprehension, and it has elicited expressions of opinions rather than facts. There has been, I think, a general tendency to hold that it is probable that, so long as any manifestations whatever exist, there must be a blood-taint. Several speakers have seemed rather to deprecate the attempt to mark out stages at all. Thus, one to whom the subject is very greatly indebted—Dr. Wilks—said that for him, when a patient had syphilis, he had syphilis; and another of high position in the profession informed us, pithily but apparently without regard to stage, that syphilis was not so much a “blood-disease” as a “flesh-and-blood disease.”

Now, in reply to those criticisms, I cannot but still think that it is consistent with fact to divide syphilis into stages; and that the degree in which the blood and tissues are relatively affected by it differs very much at the different periods. No one will probably deny that syphilis, whilst breeding in the early periods of chancre is a tissue-disease only, its yeast is not yet free in the blood; then follows a period when the blood is its home, as may easily be proved by inoculation experiment. The blood cannot be infected many days—probably not many hours—before the poison finds its way into the tissues; and the misapprehension to which I have just adverted consists in this, that some speakers have seemed to suppose that by blood-disease was meant one in which the blood alone shares. I really cannot admit that the terms I used afforded any ground for such misapprehension; for the very pith of my argument as regards the symmetry of the phenomena in the secondary stage was that the tissues became infected by the germ-carrying blood. Of course, in its secondary or exanthem stage, syphilis is a flesh-and-blood disease, or rather, to denote sequence accurately, a blood-and-flesh disease. But, in the primary stage, it was local; and it is very possible that in the tertiary stage it may be local again. In other words, the poison or yeast may have died out of the rapidly changing blood; whilst it, or perhaps its results, are still present

in the solid and less mutable tissues. To determine this, we want not opinions, but facts.

Now, there are several kinds of facts by which the state of the blood in syphilis may be estimated. First, we have its contagious properties, or inoculability. This is conclusive; and it is quite certain that, during the secondary stage, the blood is contagious. The numerous accidents in vaccination and otherwise which have occurred during this stage have fully proved, both for the acquired and inherited forms, that the blood may be rich in contagious material, even when the patient displays no external symptoms. Although, as a rule, when the blood contains the poison it will produce an eruption, it by no means invariably does so; and it may be admitted as highly probable that the blood continues to be contagious for a certain period after the external phenomena have ceased. How long is that period, and within what limits may its duration vary? That is the question before us. Most unfortunately we are precluded from experiment; for syphilis is with great difficulty communicable to the lower animals, and it is not easy to find conditions under which, in the human subject, such procedures would be justifiable.

Availing ourselves of such facts as accident throws in our way, I believe we can produce but little evidence in favour of prolonged contagiousness of the blood. All the accidents occur during the year or eighteen months which we count as the secondary stage, and most of them in the early part of it. There is every reason to believe that, in the tertiary stage, neither the blood, nor even inflammatory secretions produced by sores which still bear the specific type, can reproduce the disease. Our next test is the possibility of transmission to offspring; and I note that almost every speaker has been inclined to assert that the production of a tainted child must be regarded as proof of blood-poisoning still extant in the parent. This may be so; but I cannot help the conjecture that it may be possible for the germs to still hold possession of cell-structures in the ovary or testis when they no longer exist free in the blood. That such is the case cannot be proved, and must rest for the present as mere conjecture. It becomes, then, of great importance to answer the question—*How long after the secondary stage is it possible for syphilis to be transmitted hereditarily?* During the last few weeks, I have gone through the notes of a great many cases, in order to get data for a safe reply on this head, and

with the result of a strong impression that we have much exaggerated our estimates of the time. The cases in which syphilis is transmissible by inheritance for more than a year or two after its secondary stage appear to be very exceptional. I possess notes of a few cases in which successive children, during a period of seven or even ten years, have presented evidence of taint ; but the ordinary course certainly seems to be that the first two or three children suffer, and that the others escape. Unless the risk of hereditary transmission did really cease early, in the vast majority of instances, infantile syphilis would be far more common than it is. It will be readily seen that our decision on this point is not a mere matter of speculation or of transcendental pathology, for upon it must rest the advice which we give our patients in reference to marriage. Now, I have for long made it a rule when consulted on this point to insist that, before marriage, a period of two years should elapse from the last of what I have considered blood-symptoms. I have given this opinion to a great many persons, and may confess that it has been a constant source of anxiety, lest some day some one should bring me a snuffling, spot-covered baby, and say : " See here ; you said I might marry ; just look at this ! " Such an occurrence has, however, never yet happened to me. I may strengthen the bearing of this fact by adding that I have been cognisant of not a few cases in which the marriage took place at a much shorter interval than had been advised, and yet healthy children were produced. I repeat, then, that there is reason to believe that the instances of liability to transmit to offspring, extending over periods of several years are exceptional, and are by no means to be dealt with as if they illustrated the rule. There are, moreover, numerous fallacies to be carefully kept in mind in investigating cases of the supposed unusual prolongation of this risk. There is the almost certainty to which I shall have to allude directly, that the mother becomes contaminated by her foetus, and thus, if healthy before, supplies a new starting-point for the infection of future children ; and there is always the risk that one or other of the parents may have contracted the disease a second time.

Thus, then, we have clearly a period of syphilis during which the original sore is contagious ; a period during which the blood and tissues are contagious ; and a period during which transmission to offspring is possible. It is doubtful whether or not the two latter cease simultaneously, but there is some probability that the last

remains the longest; both, however, in almost all instances, end within comparatively short periods.

Let us ask, next, how it is with the symptoms which we rank as tertiary. Here we find a totally different law. The liability to them persists after the longest periods of apparent immunity, and after a whole family of healthy children have been produced. Nor are such cases rare; they are almost the rule. I am obliged, then, to again ask the question—Is it not far more probable that such symptoms result from changes in the solids which have taken place during the protracted secondary stage, than that they are consequent on a blood-taint still in activity.

At this point, the argument receives, I cannot but think, great support from the general fact that everything in the secondary stage is symmetrical, and everything in the tertiary unsymmetrical. The inference suggested from this is that the phenomena of the tertiary stage, not unfrequently single, are of local origin, and possibly not unfrequently acknowledge accidental causes; whilst those of the early are due directly to the circulation of poisoned blood. Now, I must confess that I have been a little astonished at some of the opinions which have been expressed during the debate on these facts as regards symmetry and the inferences from them. One speaker, if I did not misunderstand him, thought it an original statement that symmetry after all did not mean anything more than that the poisoned blood circulated equally on the two halves of the body. Now, so far from this explanation being novel, it is precisely that which gives the symptom its value. Others have alleged that, after all, secondary syphilis is not so very symmetrical, and Dr. Moxon has explained to us that, although it is symmetrical, yet its symmetry has no meaning, since it is invalidated by the "fallacy of universality." To those who doubt the fact of its usual symmetry, I have to say simply, strip your patients and inspect them in a good light; and, if this be done, I cannot conceive that there can be any doubt on the matter. Allowance must always be made for slight deviations from exact parallelism, for neither the spots on the wings of a butterfly nor the markings on a spider are ever absolutely symmetrical. It is recorded of a Dutch gardener that he caught a boy stealing apples, flogged him, and shut him up in one of his summer-houses; and that, having done this, his mind was so much disturbed by the interference with the symmetrical plan of his garden, that he could get no peace until he had flogged his own boy

and locked him up in the corresponding building. I was careful in my introductory remarks to say that we must not expect from Nature symmetry of the Dutch garden type. That in secondary syphilis the sameness in extent and location of the phenomena on the two halves of the body is such as ought to be quite conclusive as to its meaning, I still fearlessly assert. To Dr. Moxon, I reply that, if an eruption be so universal that you cannot judge of its symmetry, the fact of universality teaches pretty much the same lesson, and cannot often be explained on any other hypothesis than that of blood-contamination. Those, however, who are familiar with the syphilitic exanthem, know that it is not often universal, and that its symmetry is constantly seen in the most definite manner in cases in which it is only scanty. Sir William Jenner, directing his arguments in the same direction, has told us that the rash of typhoid fever, a blood-disease, is not symmetrical; whilst common psoriasis, the patches of which are usually symmetrical, is a skin-disease only, and may be produced by local causes. Now, there are few authorities to whom I would with greater pleasure yield my opinion, if I could, than to Sir William Jenner, but in this instance I am obliged to dissent somewhat from both his statements. If the rash of typhoid be really not symmetrical in any other sense than that it is often ill-characterised and difficult to see, it is a very extraordinary fact in pathology, and is well worthy the most attentive scrutiny of this Society. On the other hand, taking psoriasis as the very type of a symmetrical eruption, I must assert that it is probably not a mere skin-disease, and that it cannot be evoked by local causes. The influence of varying states of health and age, of lactation, &c., in producing it, and its cure by arsenic seem to prove that it is constitutional in origin; whilst if it were ever due to local causes, it could certainly never, under such circumstances, become symmetrical unless the irritation were equally applied to the corresponding parts.

Amongst those who have produced evidence in favour of the assertion that the phenomena of tertiary syphilis are, as a rule, non-symmetrical, my thanks are chiefly due to Dr. Moxon, whose facts derive additional value from the circumstance that he is an unwilling witness. Could anything be more conclusive as to the general fact on this subject than that a pathologist so able and zealous as Dr. Moxon, with Guy's Hospital as his field, mentions to us four cases of symmetrical tertiary syphilis, and four only. If the facts were as

he wished to imply, that no great difference in this respect could be established between secondary and tertiary symptoms, he would easily have been able to give us four hundred. I have myself repeatedly published cases of symmetrical tertiary syphilis, but the interest of the cases consisted in the fact that they were exceptions to the rule. It must be obvious that there is no law to prevent tissue-disease, such as I hold tertiary symptoms to be, from being symmetrical; on the contrary, the wonder is that they are not more often so, resulting, as they do, from long-standing disorder of nutrition which was caused at a time when the blood was tainted. So much the more significant is the clinical fact that, as a rule, they are not symmetrical. In repeating this assertion of their almost constant non-symmetry, I am speaking of the later tertiary phenomena, and chiefly of such as paralysis of the nerves of the eyeball or face, gummata in the tongue, muscles, or cellular tissue, and periosteal nodes.

Having mentioned Dr. Moxon's clever and amusing speech, it may be convenient here, perhaps, to conclude my reference to it, and to say that I am glad to find that he supports the exanthem doctrine of syphilis. I spoke of syphilis as a slow-staged or long-staged exanthem, and he calls it an "exanthem diluted by time." As to his suggestion that tertiary growths have their seat in portions of new matter, which, by some means, have got introduced into the system without parentage, and, therefore, without inheriting any degree of immunity, I shall prefer to avail myself of its author's permission to say nothing more about it, and shall await with interest its further development.

It has been prominently mentioned, as if it constituted a real difference between the exanthems and syphilis, that the former are not capable of transmission to offspring. The reply to this is, that such transmission is possible only during the period when the specific poison is living in the blood and tissues; and that this period in the more acute exanthems is very short, whilst the patient's condition during it is such as to utterly preclude the possibility of his becoming a parent. We cannot too clearly keep in mind this fact of extreme difference in length of stages, for by it are to be explained not only the possibility of hereditary transmission, but the subsequent liability to tertiary disease.

The statement made by a high authority, that syphilis differs from the other exanthems in having no pyrexia, I may conveniently answer

by quoting from Dr. Buzzard's speech at an earlier meeting. Dr. Buzzard, apparently as familiar with the earlier or surgical stages of syphilis as he is well known to be with those in which the nervous system suffers, spoke of fever as being "so significant a part." In point of fact, the pyrexia in the exanthem stage of syphilis sometimes runs high, and is probably always present more or less. I have known a patient almost die during it.

Amongst the important matters brought prominently under notice during this discussion, is the fact that there exists two schools of opinion as regards what takes place in the transmission of syphilis from parent to child. In the past, some, perhaps most of us, have held in a muddled sort of way both doctrines, and have allowed them to mix themselves up in a somewhat incongruous manner in our expressions and forms of belief. I cannot but regard it as a great gain that the two are now fairly confronted, since, without presuming to assert that they are incompatible, it seems very improbable that both are true. The one, which is the creed of those who think that syphilis is due to a sort of yeast germinal matter, holds that transmission is effected only by transference from parent to child of germs; that it is, in fact, contagion to the ovum. Thus, unless such infectious matter pass with the sperm or germ, no transmission of the disease will take place, and a child perfectly healthy, *quoad* syphilis, may be born. Transmission thus becomes to a certain extent a matter of chance, and by no means subject to the laws of ordinary heredity. The other view which, with the clearness and ability which characterise all that he teaches, was expounded by Sir William Jenner, makes the transference of syphilitic taint from parent to child but an example of heredity, in the same way that colour of hair, texture of skin, &c., are transmitted. "Potentiality of development, then, and not the state of the blood, not any appreciable change, is the real pathology, it seems to me, of infantile syphilis." I am quoting Sir William's exact words.

Let me try to state as briefly as I can a few of the facts in reference to these two doctrines. When syphilitic parents procreate, a not uncommon result is death of the ovum at very varying periods of intra-uterine life, a circumstance, probably, to be explained more often by disease of maternal structures (of the placenta) than by breeding of syphilitic virus in the infant. In another group of cases the infant is born apparently healthy, but sickens rapidly and, without any of the ordinary evidences of syphilis, but with pos-

sibly a pemphigoid eruption on the hands and feet, dies in spite of all treatment within a few days. It is very difficult to say what happens in these cases, or in what way death is caused; they are rare. Lastly, we have the common, well-known result, that a fine healthy infant is born at full time, which at the end of a month begins to snuffle, and a few weeks later shows a symmetrical rash closely similar to those seen in the acquired syphilis of adults. These symptoms are, as in the adult, amenable to treatment, and also subject to the law of spontaneous disappearance after a definite duration. It is remarkable that the period which intervenes between birth and the full development of the exanthem stage is exactly that occupied by incubation in the case of acquired disease, and the conjecture is forced upon us, that the development of the yeast in the child's blood is restrained during intra-uterine life, and commences only with the function of respiration. The phenomena which ensue are those of a specific fever, and in no respect resemble those of development. If aberrations from normal development are ultimately produced—peculiar physiognomy, malformed teeth, &c.—they are always in ratio with the preceding infantile inflammations, and are probably directly produced by them. If a syphilitic child in infancy escape syphilitic inflammation, its development will probably be in no respect peculiar; it may grow well, and present every appearance of excellent health. Thus far, then, we may assert, that what happens to a syphilitic infant is closely similar to what occurs in the acquired disease, and suggests rather breeding of specific poison in the blood and tissues, than the inheritance of "potentialities of development." It may be added that, were the latter creed the true one, we ought to find children inheriting the disease at the special stage to which it had attained in their parents; whereas, this old opinion is now generally discarded, and it is acknowledged that, whatever may be the stage in the parent, the child will begin again at the early secondary period. If the theory of yeast-contagion to the ovum be the true one, we ought to find no shading-off in degree of severity of transmitted taint—the child must inherit the whole of syphilis or none; whereas, if the other view be correct, we should expect the most various gradations, the severity being in relation with the parent's state. Now, I believe that the general—perhaps the universal—opinion held at the present time is, that inherited syphilis does really shade off, so to speak, in the younger children. On former occasions, I have myself used expressions favouring this

view, and, if I now discredit it, I fear I may encounter suspicion of allowing my theoretic belief to influence my interpretation of fact. I may, however, assure the Society, that, although I have admittedly changed my opinion on the suggestion of theory, I have not allowed myself to do so without collating very carefully the evidence afforded by the histories of some scores of syphilitic families. It appears to me, as the result of this study, that there is no proof of the transmission of minor degrees of syphilis, and that what we witness in children is rather exactly parallel with what we observe in the acquired disease, *very variable severity in different individuals*. If the real facts as regards the natural history of syphilis could be obtained, we should probably find that a considerable proportion of those who pass through the syphilitic fever after chancre-contagion, do so without rash, without sore throat, without, indeed, any visible symptoms, and that this is by no means always dependent upon the treatment, but rather upon some peculiarity in the patient's organization, or some arrest in the development of the yeast poison. If this be the case in the acquired disease, we need not wonder that many infants who yet show evidences of taint in later life escape the ordinary rôle of symptoms during the first few months of the first year. I have notes of many families in which some suffered and others appeared to escape; and although, as we should expect, it is the rule for the eldest to suffer most severely, there are so many exceptions to this, that I do not think we can infer anything as to diminished intensity of the poison. We ought rather to say, that it is the rule for the eldest to suffer most often, and that the chance of total escape is increased with each successive year. As the rule, the younger members of syphilitic families show no traces of the disease whatever, and appear to have escaped entirely. Lastly, it may be remarked, that if the inheritance of syphilis were an inheritance of "potentialities of development," and not, as Mr. Simon has well expressed it, of "a material something which passes absolutely and bodily into the infected ovum," we should expect to have mixed and ill-defined results. We should see the syphilitic potentiality mingling itself with others and producing hybrid diseases. And, further, one scarcely sees how on such an hypothesis a syphilitic parent should ever have healthy children, or why his younger ones should suffer less than his early ones. Admitting that the subject is one of much difficulty, and that further investigations of fact are needed, my conclusion still is for the present a tolerably confident

one, that the transmission of syphilis to children is like that from person to person in the acquired disease, a communication of special germs, and that there is no such thing as the transmission of less or more—that the foetus either gets syphilis in the full or not at all.

I confess that it seems very difficult, taking this view of the mode of transmission, to believe it possible that the germs received *in utero* can survive long enough to be transferred to a third generation. For this to take place, a period of vitality of nearly twenty years must in most cases be presupposed, and this is, I believe, far longer than can be proved in the case of the acquired disease. Facts bearing on this point are very few in number, and most of them negative. Mr. Simon mentioned briefly the other night one which seemed to favour it. Some years ago I recorded a number in proof that those who had suffered severely might yet bear healthy children, and with them one which might be considered to bear in the other direction. The fallacies are, however, almost insurmountable.

Whilst urging that the kind of poison communicated in parental transmission is probably just the same as that conveyed in chancre-contagion, we must not forget that there are possibilities of difference, to fully appreciate which a knowledge of the laws of cryptogamic life, rather than of heredity, is needed. To this important topic Mr. Simon and Dr. Greenfield both alluded; but I shall be best able to illustrate what is meant if I may be allowed to pass back to the speech of Dr. Broadbent. Amongst certain difficulties in the way of accepting the theory that tertiary deposits occur only where secondary disease has preceded them, Dr. Broadbent mentioned the well-known opinion that a healthy woman pregnant by a syphilitic husband may acquire the disease from the foetus, and pass at once to the tertiary stage, having never suffered from any secondary symptoms whatever. My reply to this would be, that in such cases the "tertiary symptoms" are never displayed early; that they follow, as in other instances, after long intervals; and that the cases are probably examples of the secondary stage being passed through without external signs. The subject is, however, far too important and too curious to be thus dismissed. If what seem to be facts respecting it be really credible, we appear to have a wholly novel phase of the disease opened out to us for investigation. It has long been believed by many that the mother might become diseased by absorption direct from the blood of the foetus, and without the intervention of any chancre. It is just twenty years since I

wrote a paper on this subject, and collected a good deal of evidence concerning it. My conclusion was, as stated by Dr. Broadbent, that in these cases the woman never suffers from secondary symptoms, and experiences nothing during the pregnancy which infects her, excepting perhaps a little loss of strength and slight indefinite ailments. In further proof that she does really contract something, I made use of the well-known observation of Abraham Colles that syphilitic infants nursed at the breast often infect wet nurses, but never their own mothers. I did not, however, until within the last few weeks, fully appreciate the bearing of this fact. If Colles's law be one which has no exceptions, it follows that all women who bear syphilitic children contract syphilis; for how else can they obtain immunity? And, since it is notorious that women under such circumstances scarcely ever show secondary symptoms, it follows, further, that we have here a form of syphilis which is protective, but which is unattended by any cutaneous outbreak. Thus, syphilis acquired by blood-contagion from the fœtus would appear to be, for the mother, a parallel with vaccination in regard to smallpox: she gains immunity without suffering from any severe form of disease. The botanist will at once suggest that probably in both cases the explanation is to be found in heteromorphism or alternation of generations on the part of the fungus. To him the clinician might aptly rejoin, that really one might almost have expected it; for, when the mother gets syphilis from the fœtus, she obtains it from fluids in which the plant-life is evidently under some very special restraint; for in the fœtus itself, as a rule, no development of it takes place during the nine months of intra-uterine life. We have only to suppose that the same condition of the yeast which existed in the fœtus is perpetuated in the mother, and the thing is done. We had no right to suppose that infection by inoculation of solids (or chancre-contagion) would be exactly the same in its results as direct imbibition by the blood.

As we may call vaccinia undeveloped yet protective variola, so we may consider syphilis, derived from the fœtus, as an undeveloped yet protective form of that disease; and we have here another most interesting point of analogy between syphilis and the exanthems.

Before, however, we accept as probable such a possibility as that just hinted at, it is desirable to look at the facts with the utmost incredulity. Let us doubt unsparingly at every stage of the reason-

ing. First, is Colles's law true? I cannot see any escape from the conclusion that it is. It was announced in 1837, and has received, I believe, the assent of every authority who has written on the subject since. It has attracted attention both at home and abroad, and I am not aware that a single exception to it has been recorded. We have all of us seen chancres on the nipples of wet nurses. They are, indeed, not very unfrequent. We have, however, none of us seen such on those of the mothers of infected children. Let us remember that it is very unusual to put a syphilitic infant out to wet nurse—a thing which no prudent surgeon would ever permit—and that probably, for one so nursed, a hundred are suckled by their mothers; and we shall appreciate the weight with which this entire absence of proof that mothers ever suffer bears. It amounts, I think, to all but proof that they are absolutely insusceptible. It is as strong in that direction as is the rarity of small-pox within short intervals after successful vaccination. We must remember also that these mothers of syphilitic infants not only nurse one infected child, but often several in succession; that they not only suckle them, but handle them, dress their sores, and in various ways through long periods expose themselves to risk. If it be granted that it is proved that these mothers—a very numerous class—have really in some way had syphilis and acquired immunity, I do not think there can be much dispute as to the next fact, that they do not during pregnancy show any of the usual symptoms of the disease in its secondary stage. This is a matter of everyday experience. There remains, however, the possibility that the syphilis may have been gone through prior to pregnancy; and I am well aware that the few remaining writers who teach that syphilis can be inherited only from the mother will hail this confirmation of Colles's law as a strong support for their opinions. Here, however, again I must appeal to everyday experience. Is it not the fact that women bear syphilitic children without having ever themselves, either before or during pregnancy, had any symptoms, either primary or secondary, of that disease? If this happened only once or twice, we might reasonably doubt the histories given us. But it is not so; it is in hundreds of cases; and few, I think, of much experience, can doubt that, as a rule, syphilis is inherited from the father; and that the mother never shows any external signs of the malady. I purpose shortly to publish the evidence which I have collected on this and some kindred subjects; and the assertion just made will, no doubt,

receive the scrutiny of other observers. If the argument should in the end be thought to be substantiated, some other very interesting questions will suggest themselves. We shall have to ask—What are the ulterior liabilities of a woman who has thus acquired the modified form of syphilis? Can the taint be transmitted from her to offspring borne subsequently to a healthy father? Is her blood at any period contagious; and what would be the result of inoculation with it? Does the taint in her last as long as when the disease is acquired in the ordinary way? I have already, in speaking of the difficulty of measuring the length of time during which it is possible for the tendency to transmit to offspring to last, adverted to the fallacy introduced by the fact that usually the mother acquires the disease from her first pregnancy, and may herself become the source of contamination in the second. We must not, however, take it for granted that a taint acquired in this peculiar manner is transmissible, or, at any rate, that it is so for any long period of time. It is quite possible that its stages may be far shorter than those of the common type of the disease.

Here, Mr. President, I will ask permission to leave the proper topics of reply, and to make a suggestion. It is this, that our next debate should be on the laws of evolution of the exanthemata. Surgeons have been asked to make a clean breast of it as regards syphilis; and we have told all that we know, and much, perhaps, that we only half know. Let us next solicit of medicine a similar declaration of faith in reference to the most interesting part of her proper domain. There are a host of questions upon which we surgeons, out of pure scientific curiosity, and with no wish to intrude into medical practice, are longing to be enlightened. By what right does diphtheria claim its place as a specific fever? If the rash of typhoid be not symmetrical, on what pattern and by what influences is it arranged? What is the interpretation of the curious phenomena of corymbiform variola? If a pregnant woman suffer from an exanthematic fever, what are the possible events as regards the fœtus? and does it ever happen under such circumstances that the infant develops the disease, as in the case of syphilis, soon after it gains access to the light? Are not the stages of the exanthemata far less accurately determined and much more liable to variation than is generally admitted? May not varicella, for instance, vary in its incubation period from fifteen to twenty-seven days? and in its exanthem period from four (the average) to fourteen or twenty days?

—limits of variation wider than we need claim for syphilis. Is it possible to convey the disease by inoculation with varicella-fluid?—a fact denied by most writers, but affirmed by one of the latest without citation of any evidence. Is it not probable that cases are very common in which patients go through specific fevers and acquire protection, without having had any rash or shown any external symptoms? On these and many other important topics medical opinion is, I believe, very far from being settled; and their investigation might, perhaps, have the collateral advantage of diverting the zeal of our medical friends (excepting in the *post-mortem* room, where they are always welcome) from the one specific fever, which is all that the surgeon can claim as his. To speak seriously, it has sometimes struck me forcibly that, in comparing syphilis with the exanthemata, the large amount of uncertainty which still surrounds the latter is a most serious bar to progress.

Amongst the more important of the questions which have been put to me in the course of the debate, I find one in the very suggestive speech of Dr. Greenfield, as to whether there may not be some after-results of syphilis which are to be classed rather as those of malnutrition than as consequences of the specific poison. He illustrates the question by asking whether all the children who suffer from keratitis have really had syphilis, and whether it may not, in some cases, rank rather as a defect in nutritive power. To this I may give a clear reply, that I believe that all the conditions which we have as yet recognised as syphilitic, whether in the acquired or the inherited form of the disease, are the direct results of the disease. A severe attack of syphilis may of course damage a man's nutritive power, but as the result of such damage we shall not encounter anything in the least special; and it is quite impossible that he should, in virtue of such, transmit to his children a state of cachexia rendering them liable to such a disease as interstitial keratitis. The latter malady cannot, I feel convinced, occur to any one who has not had syphilis. Interstitial keratitis also obeys the usual law of syphilitic inflammations in always showing tendency to spontaneous cure, which would scarcely be the case if it resulted from defect of nutrition. There is, however, a rare form of choroïdo-retinitis, which is steadily and slowly aggressive, much like a degenerative affair; but I suspect that it is secondary to damage done by previous inflammation, otherwise it might seem to be an example of what Dr. Greenfield is seeking.

The peculiar relationship in which the gumma, the soft sore, and phagedænic action, stand to syphilis, have perhaps been sufficiently discussed. I may note, however, that, with the exception, I think, of Mr. de Méric, no speaker has attacked the dogmatic assertion which I very purposely made as to my belief in the essential alliance between phagedænic and syphilitic inflammations. I was very anxious to attract attention to this subject, believing it to be of great clinical importance. I have long held, and publicly taught in the most positive manner, that, certain very rare cases excepted, all well-characterised phagedæna may be traced to syphilis; and that it is from the contagion by pus from syphilitic sores (mostly tertiary, and not containing the specific virus) that hospital phagedæna takes its rise. You, Mr. President, can, I know, give us some valuable evidence on this point; and as I hold that its knowledge is of great importance, in order to the prevention of epidemics of phagedæna, both in military and civil hospitals, I rather regret that it has not, on the present occasion, had the benefit of more criticism. Mr. de Méric had somewhat misunderstood my meaning, and seemed to think that I held syphilitic phagedæna to be incurable, which was very far from what I intended to say. I should be very glad if I could believe that the silence of other surgeons implied acceptance of my creed.

As a curious illustration of the difficulty in making one's meaning clear, I find that I have been criticised by Mr. Wood and others for attempting, as they think, to define too closely the stages of syphilis, whilst Mr. de Méric complains that I have shown too great a tendency to put the whole disease in one lump, and expresses his preference for the doctrine of stages. I cannot defend myself on this point from either critic without recapitulating almost the whole of my address—a task which the Society will, I am sure, gladly excuse me. Briefly, however, I may say that I consider the several so-called stages of syphilis as in part natural and in part conventional, and that I have not the slightest wish to make them more definite than is required by clinical convenience or suggested by pathological probability.

Amongst the chief contributions to facts, we must place Dr. Buzard's valuable statistics as to the average age of those who suffer from disease of the nervous system, Dr. Hilton Fagge's statements as to the frequent alliance between syphilis and amyloid disease, and Dr. Greenfield's account of certain pathological details. Dr. Moxon

has also mentioned some interesting examples of symmetrical tertiary growths ; and Dr. Greenfield has recorded a *post-mortem* examination in the secondary stage, in which gummata were found in the dura mater. I am sanguine that, hereafter, many of the topics which as yet have been only debated, will be examined by the light of accumulations of new facts, otherwise some regret might perhaps be felt that, following, perhaps, my own bad example, most speakers have contented themselves by expressions of opinion.

The possibility of diagnosing syphilis by the odour of the patient was adverted to by Sir William Gull. The late Mr. Wormald used to teach that this was possible, and was fond of saying, "They can't deceive me ; I nose 'em." It probably requires a very good sense of smell to make much use of this symptom. For myself, I may confess that I have rarely been able to distinguish anything peculiar in the odour of adults suffering from syphilis ; but in infants I think it is often very perceptible and quite special.

In answer to the numerous surgeons who have requested further details as to my disbelief in "dualism," I may briefly say that, although of course we must all fully recognise the differences between hard and soft sores, yet I do not think that the soft is a special form of disease. On the contrary, I believe that it is usually produced by contagion with the results of syphilitic inflammation ; and that, if we could get rid of true syphilis, we should after a while cease to see examples of the soft sore also, with the exception of those which result from common inflammation, balanitis, and the like. My views on this point are explained in detail in a lecture published in the 'Lancet' six months ago. To those who were inclined to complain that I had said that dualism was "dead" whilst it is still mentioned in several standard and excellent works, I can only reply that I was careful to say only dead, and that I did not say that it was buried. Had I done so, I should have been a little premature.

The observation made in the address as to the extreme infrequency of affections of the brain and nerves in inherited syphilis has been confirmed by Dr. Buzzard and all speakers who have adverted to it. As none have mentioned facts in opposition, I think we may now regard it as generally admitted. I am not sure but that some statements which have been made may even help to explain it. Dr. Buzzard and Dr. Greenfield have both called attention to the probability that the tertiary growths take place in perivascular spaces and other adjuncts of the lymphatic system. Professor Sigmund of

Vienna has also, during this discussion, in a paper published in the 'Practitioner,' drawn attention to the circumstance that syphilis, from its beginning, shows an especial tendency to develope in lymphatic structures. Now, we must remember that this is true only of syphilis when acquired by a chancre. It is not the case when it is transmitted from parent to child, nor when a foetus infects the blood of its mother. In these, there is in the beginning no implication of glands. This may possibly be the reason why in neither of these classes of cases do we find in the later stages of the disease that liability to peri-vascular growths and gummata which are not unfrequently witnessed as results of the chancre form of syphilis. It appears highly probable that we have to deal with syphilis in two or three (perhaps more) different moods, varying according to the precise manner in which the transference of the yeast-poison is effected, and the structures into which it is first introduced. In connection with these differing moods, different classes of sequelæ must be expected.

Dr. Wilks, Mr. Berkeley Hill, Dr. Buzzard, and others, have adverted to the interesting fact that serious tertiary symptoms are not unfrequently met with in those who have suffered but very little in the earlier stages. This is to be explained probably by several different considerations. In the first place, it is true only of a certain class. Some of the very worst examples of tertiary disease in which multiple manifestations occur, happen often to those who have suffered throughout very severely, who have perhaps scarcely ever got rid of their symptoms. Such at least has been my experience, and these are often patients with whom the specifics have disagreed, or who show, in particular, an idiosyncrasy as regards mercury. They are often very seriously out of health. There is, however, another class in which the tertiary symptoms attract much attention, and become very serious, not so much on account of their own extent or severity, but because located in an important position. A patient apparently in perfect health may have a single gumma on his brain, which may cause him most serious distress, whilst he has not about him another symptom of syphilis, either present or past. These are the cases to which I suspect allusion was made, and in these not unfrequently the patient knows but little about his early symptoms. I suspect that the real explanation of the frequency of such cases as these, and of the converse fact that those who have had well-marked secondaries usually escape sequelæ, is that the latter are well treated by mercury, whilst the former neglect it.

I am painfully aware that in this reply I have done very scant justice to the numerous speakers who have taken part in the debate. I have, however, I hope, profited by all, and have endeavoured to incorporate in my statements to-night the hints they have given. That I have not even alluded to the splendid speech of Sir James Paget, is to be explained by the fact, that we agree on almost every point; and this, again, finds its solution in the further fact, that he was my teacher, not alone, as I was proud to be reminded, in general surgery, but also in the special doctrines of syphilis.

In conclusion, Mr. President, I have to express my sense of obligation for the many valuable suggestions which have been addressed to me both in public and in private, during the course of this prolonged debate. I should be ungrateful, indeed, if I did not very warmly appreciate also the kind and very flattering manner in which my introductory remarks have been received as well by my friends in this room as by my critics in the medical press. It is not for me to say whether the debate will add greatly to our knowledge of its subject; but I may confess with pleasure that I have myself learned much. If it have not been a harvest, we may, I think, hope, at any rate, that it has been seed-time, and that syphilis, long ago named by one of our great masters "the key to all pathology," may in the future be found yet more useful.

ON GANGRENOUS ERUPTIONS

IN

CONNECTION WITH VACCINATION AND CHICKEN-POX.

BY

JONATHAN HUTCHINSON, F.R.S.,

SENIOR SURGEON TO THE LONDON HOSPITAL AND TO THE HOSPITAL FOR
SKIN DISEASES; CONSULTING SURGEON TO
MOORFIELDS HOSPITAL.

Read October 25th, 1881.

[*From Vol. LXV of the 'Medico-Chirurgical Transactions,' published
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1882.

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Received October 7th—Read October 25th, 1881.

I HAD the honour to exhibit before the Society about two years ago, the body of an infant who had died of a severe gangrenous eruption, which had followed vaccination.

I believed that it was an example of “*Vaccinia gangrenosa*,” i.e. a vaccinia eruption which had taken on a gangrenous form. I now purpose to bring before the Society the details of the case, and also to give some account of an allied and much more common malady to which for many years past I have applied the name “*Varicella gangrenosa*.”

*Details of Case in which Death followed from a Gangrenous
Eruption after Vaccination.*

A male infant, aged about three months, and in excellent health, was vaccinated on November 11th, 1879, at a district

office. The vaccination was from arm to arm, and the vaccinifer appeared to be in perfect health. Four others were vaccinated from the same source at the same time, and nothing unusual happened. On the eighth day after the vaccination the child was brought again to the station for examination. It had four pearly vesicles on the arm, which showed nothing unusual, but its body and limbs were covered by an eruption of a peculiar character. This eruption, which had been coming out for a day or two, was described by my informant, the vaccinator, as looking like smallpox. He said that the papules were distinctly shotty, and, believing it to be a case in which variola had been contracted prior to vaccination, he instructed the mother to take the child home and on no account to bring it to the station again. Four days later he visited the child at its home and found the pustules much developed, whilst in many of them gangrene was commencing. He now advised that the attendance of the parish medical officer should be obtained. This was not done, and between this date and that of the child's death no medical man saw it. The death occurred on November 30th, twenty days after vaccination. Under the peculiar circumstances an inquest became necessary, and an opinion having been expressed that the case was one of vaccinal syphilis, I was requested by the coroner to examine the body and make a report.

The state of the child's skin after death may be realised with tolerable accuracy by the portrait (see Plate I). The vaccination spots were covered with scabs, and there was a certain amount of congestion around them, but none of them were in the least indurated.

On the scalp and face there were spots and patches ranging in size from a shot to a shilling, some of them simply congested or scabbed, but others showing a central area of gangrenous skin. On the trunk, both back and front, there were similar spots with some also much larger. All the larger ones showed a rim of deep ulceration which surrounded a central slough of black skin. From a few

the slough had separated, a deep ulcer remaining. But few of the ulcers were quite round, and in many instances the shape was irregular, as if several spots had coalesced. The eruption occurred on both sides of the body alike, but as regards the larger eschars, it was by no means arranged in exact symmetry. The back was very severely affected, and the lower part of the abdomen and upper parts of the thighs. There were some large eschars near the knees, but the lower halves of the legs and the feet, and the whole of both upper extremities below the elbows, were almost wholly free.

A post-mortem examination of the body was made for me by Dr. Barlow, who reported that the viscera were free from disease and that the child appeared to have been in a state of good nutrition.

So far as I am aware this is the first case of gangrenous eruption after vaccination which has been recorded, but that a general exanthematous rash does sometimes follow at the end of the first week after vaccination has long been well known. To this eruption the term vaccinal exanthem has been given. Several somewhat different forms of eruption have been recognised in this connection. Sometimes the rash is simply erythematous, at others papular, and in a few it closely resembles varicella. It has been described in detail by Mr. Ceely and others. I believe that the case just narrated was an example of vaccinia which became gangrenous, and it is of interest to note that the primary lesion of the skin was a hard papule with an umbilicus, and so closely resembling variola that it was taken for that disease by the surgeon who saw the case.

In a paper written some years ago, I attempted to draw attention to the fact that a peculiar form of prurigo, which is a common consequence of varicella, occurs also sometimes after vaccination, and probably in connection with the vaccinia rash. Thus it would appear that there are certain features of similarity as regards the pathological possibilities of vaccinia and varicella, and to these

I wish now to add the fact that they are both liable to become gangrenous and to end fatally.

Before I pass to the subject of varicella gangrenosa, I wish to mention another case of gangrenous vaccinia which has occurred in Dublin since my communication to this Society in November, 1879. The case occurred under the care of Mr. William Stokes of the Richmond Hospital, and I am indebted to him not only for particulars of the case, and permission to make use of them, but also for being able to bring before the Society an excellent portrait of the patient. In Mr. Stokes' case the gangrenous patches were much larger than any which occurred in mine. Those on the buttocks were several inches in length. As in my case they are irregular in shape, and evidently produced by the confluence of groups of papules. It is unfortunate that in this case there is uncertainty as to the date of vaccination and consequently as to the length of the interval before the appearance of the eruption.

Neither in this case nor in my own were the vaccination sores themselves attacked by gangrene; it was the general eruption only which was so affected. The full details of Mr. Stokes' case have been published by him in the 'Dublin Journal of Medical Science' for June, 1880.

Varicella Gangrenosa.

The earlier cases which came under my notice were mostly in the late stages of the disease, and none were so characteristic as in a later one of which the portrait is produced. In this case the patient was under the care of Dr. David Lees at the Children's Hospital, Great Ormond Street, who kindly procured me an opportunity for seeing it.

In my own cases the patients usually came under observation in the surgical practice at the London Hospital, on account of deep gangrenous ulcers in the flexures, and with a few scattered spots on the skin. It was by the history only that I was enabled to connect such cases with chicken-pox.

I am sure that I have seen five or six of this kind, but

I regret that I have not preserved notes of any in a form which would justify my producing them to this Society. I recorded in the 'Ophthalmic Hospital Reports,' vol. vi, 146, a case in which a young child after an eruption much like chicken-pox had a double irido-choroiditis, and lost both eyes. A few years later a case similar to this came under my care at Moorfields, the child being brought there on account of double iritis. The child had several scattered spots of gangrene where the vesicles had been. I suggested gangrenous varicella, but was unable to carry the proof further as there was no evidence to show that the child had been exposed to contagion.

In a lecture on some of the peculiarities of varicella, delivered many years ago, I mentioned "a form of ulcerating chicken-pox which in syphilis finds its homologue in rupia." I then quoted a passage from Trousseau, who long before had described an epidemic of chicken-pox, in which the eruption had been protracted for six weeks or two months, and had produced ulcerations like those of pemphigus.

There are in Guy's Hospital Museum excellent wax casts from two cases which I should diagnose as gangrenous varicella. They are classed under the name of rupia escharotica.¹ In the description of one of the models it is said "the earliest stage of the affection is seen in certain vesicles ranging in size from a pin's head to a split pea. Some of them are flattened and have red areola round them. Subsequently the vesicles became larger and ulcerated." It is stated in this case that the child recovered in a fortnight without having taken any medicine, a fact strongly at variance with the usual history of pemphigus. No one who has examined these models and read Dr. Fagge's descriptions, and who is acquainted with Dr. Whitley Stokes' description seventy years ago of what he called "Pomphigus gangrenosus," can doubt for a moment that the "Rupia escharotica" of Guy's and the pemphigus gangrenosus of Irish observers are one and the same malady. I hope to make it scarcely less clear that they are both of them really forms of varicella.

¹ Models 206 to 209.

Dr. Whitley Stokes, in 1807, wrote in the 'Dublin Medical and Physical Essays' a paper on "An Eruptive Disease of Children." He proposed for it the name of "Pemphigus gangrenosus" or "White blisters." He described it as very severe and frequently fatal, and said that it was well known in many parts of Ireland. It is not improbable that several different maladies contributed to his description, but that his attention had been attracted to cases of gangrenous chicken-pox is, I think, almost certain. He said that the eruption usually developed itself during perfect health, and that it occurred by preference to strong children. "One or more vesicles appear, mostly larger than the most distinct smallpox; these increase for two or three days, burst, and discharge a thin fluid and a disagreeable smell." The febrile disturbance which accompanies it he believed to be induced by the irritation of the eruption. Death usually occurred about the tenth or twelfth day, and in those who recovered relapses were common. Most of the patients were between the ages of three months and four years, but it had been observed as late as nine years. The flexures of the joints and behind the ears were frequently affected by gangrenous ulcers, and gangrenous spots were the characteristic of the disease.

Dr. Whitley Stokes distinguished the disease from chicken-pox, but he evidently came very near to the recognition of what I believe to have been its true character. In speaking of diagnosis, he wrote: "On the other hand, the swine-pox (varicella) resembles this disease in its first stage; but the fever rarely precedes the eruption in 'white blisters,' and the pustules of varicella dry quickly."

If we put aside the assumption that in all cases the vesicles of varicella dry quickly, we have no point left for differential diagnosis except the occurrence in varicella of fever before the eruption, and it is now well known that this is often, if not usually, so slight as to be easily overlooked.

Dr. Stokes mentioned that in some cases the eye

is inflamed, a fact which, as already mentioned, has fallen under my own notice. His observation as to the occurrence of spreading gangrene in the flexures is one which I can also quite confirm.

The evidence upon which I rely in support of the belief that this eruption is no other than a modified varicella is the following. It occurs to children in good health at the age at which chicken-pox is common, and it affects the parts usually the sites of that eruption. It disappears spontaneously after a short time, though on account of its local severity it is always protracted longer than ordinary chicken-pox. The eruption in most instances comes out in a single crop, all the vesicles or sores being at the same time in the same stage. In the early stage it usually closely resembles chicken-pox, and is often taken for it by the child's parents. The names which it has received in Ireland of "white blisters" and "eating hives" are very suggestive. Lastly, and I think conclusively, I have seen it in case after case in which one child in a family had this gangrenous eruption, whilst the brothers and sisters had chicken-pox in the ordinary form. Dr. John Abercrombie, of the Children's Hospital, in February, 1880, brought before the Pathological Society the dead body of a child which offered a good example of the malady, and mentioned that after the child's admission at the hospital one of its brothers developed ordinary varicella.

The constitutional disturbance which attends this form of eruption frequently runs high. Dr. Whitley Stokes spoke of the disease as being often fatal, and I have myself known of several deaths. The cure, however, when healing commences, is usually rapid; all the sores, as a rule, cicatrising simultaneously.

In explanation of such exceptional severity of a disease, so common and usually so insignificant, it is impossible to do more than fall back on the suggestion of individual idiosyncrasy. The severe form does not happen to delicate children more frequently than to the robust, indeed, in

several instances in which three or four brothers and sisters had varicella at the same time, whilst in only one did the eruption become gangrenous, the subject of this latter form was by no means in more feeble health than the others. I have never seen more than one child in the same family affected by the gangrenous form.

In conclusion, I should attempt a summary of the statements which I wish to offer for the consideration of this Society.

I have tried to prove that it is possible for the eruption of varicella in isolated cases and in connection with idiosyncrasy on the part of the patient, to assume a very severe type, becoming bullous, petechial, or even gangrenous. In these gangrenous forms there is much constitutional disturbance, and death may result. Now and then very dangerous forms of iritis or of panophthalmitis are witnessed. Next to the assertion that there is such a malady as gangrenous varicella and in part based upon it, comes a second proposition to the effect that the *vaccinia* eruption may also assume the same type and with similar danger to life. That there does occasionally occur after vaccination a general eruption all observers admit. I have adduced only two examples of the gangrenous form of this eruption.

Respecting my own case I do not think that there can be any reasonable doubt. The child was in excellent health, was successfully vaccinated, and was affected seven days afterwards by a general eruption which was taken for small-pox. This eruption became gangrenous, and the child died of exhaustion on the twenty-first day. The eruption may have been variola, varicella, or vaccinia; that it was one of these three is almost certain. In Mr. Stokes' case the facts are similar, with the exception that it is impossible to assign the period of incubation correctly. That the vaccination sores themselves remained in both cases free from gangrene has been alleged by some critics of Mr. Stokes' case as a reason for doubting whether there was any connection between them and the eruption. But

such doubt is, I submit, not reasonable. It is the exanthem, and not the site of inoculation, which is attacked by gangrene, and surely there is no *à priori* reason for expecting that the latter should suffer.

In syphilis a very severe form of rupial eruption may follow in cases in which the original chancre was a mild one. In my own case the death of the patient deprived us of an opportunity of observing the healing of the sores, but it is to be noted that they had all at the same time advanced to pretty much the same stage. In Mr. Stokes' case the healing of the sores simultaneously gave support to the belief that they were of the nature of an exanthem, and not in any sense accidental results.

APPENDIX.

The following are descriptions of some cases not mentioned in the paper and of drawings which were exhibited at the meeting.

1. A portrait lent me by Dr. Barlow, from the collection in the Children's Hospital. The infant died with an eruption consisting of gangrenous patches exactly like those shown in the previous and following portraits. No history as to varicella has been preserved.

2. A portrait lent me by Mr. Waren Tay, showing the character of the eruption in the chest of a young child. Two round sores are seen, each with a central eschar of gangrene, as if punched out. In the first instance the child presented an eruption of varicella, which was seen and diagnosed by Dr. Sansom, of the North Eastern Hospital for Children. A considerable number of the spots subsequently became gangrenous, and passed into the stage shown in the portrait. Mr. Tay was kind enough to give me an opportunity of seeing the child just before the portrait was taken. I am not able to state with positiveness any facts as to family history, but I believe that varicella had occurred in the family.

3. A portrait lent me by Dr. Barlow from the collection in the Children's Hospital, showing varicella as an ulcerating, bullous eruption, not absolutely gangrenous. This portrait is of great interest as illustrating a lesser degree of severity in the inflammatory process. It shows well how large irregular sores have been formed by the coalescence of groups of bullæ. The shapes of these sores are exactly like those seen in gangrenous cases. The severity of the eruption on the trunk and head, and the comparative exemption of the extremities, is also well seen. This probably well illustrates the eruption described by Trousseau as pemphigoid varicella.

4. Portrait of an infant aged eight months, named Sawyer, who came under my own observation in August, 1880, in the condition shown in the sketch. There was a history of gangrenous varicella six weeks previously. The portrait was taken in order to show the kind of scars which are left by this eruption, and to show that they might easily be mistaken for those of syphilitic disease.

The following are some particulars respecting other cases not mentioned in my paper.

Dr. David Lees gives me the particulars of a case of the child, George Watson, aged fourteen months, who was under his care as an out-patient in January, 1880. Varicella had appeared twelve days before, and four other children in the same house had it. In this child the spots had developed into deep sores, and in some on the parietal region there were black eschars appearing. One on the left cheek was so deep as almost to involve the mucous membrane. A subcutaneous abscess was formed in the neck.

Dr. Lees also gives me the notes of a child aged a year and a half, named Sarah Ann Reed, who in December, 1879, had a vesicular eruption, from which resulted a number of punched-out sores on the lower part of the back.

Also a third case, in which a female child, aged nine months, under Dr. Dickinson's care, had undoubted

varicella, had subsequently deep ulcers on the upper and inner parts of the thighs. There were no actual sloughs, but foul ulcers as large as sixpences resulted.

DESCRIPTION OF PLATE I.

(Gangrenous Eruptions in connection with Chicken-pox and Vaccination. JONATHAN HUTCHINSON, F.R.S.).

A portrait of the back of the child whose body was shown to the Society in November, 1879, and in whom eruption resembling variola appeared on the sixth day after vaccination. The drawing was taken after death, which had occurred from exhaustion on the 20th day. It will be seen that the vaccination spots, four in number, are somewhat inflamed, and show pus scabs, but they are not gangrenous. The trunk and upper parts of limbs are covered with gangrenous patches.



A SECOND REPORT
ON THE
COMMUNICATION OF SYPHILIS
IN THE
PRACTICE OF VACCINATION.

WITH TWO ADDITIONAL CASES.

BY

JONATHAN HUTCHINSON, F.R.C.S.,

SENIOR SURGEON TO, AND LECTURER ON SURGERY AT, THE LONDON HOSPITAL.
SURGEON TO THE MOORFIELDS OPHTHALMIC HOSPITAL AND TO
THE BLACKFRIARS HOSPITAL FOR SKIN DISEASES.

*[From Volume LVI of the 'Medico-Chirurgical Transactions,' published
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1873.

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AND TO THE BLACKFRIARS HOSPITAL FOR SKIN DISEASES.

Received January 16th—Read January 28th, 1873.

IN the spring of 1871 it became my duty to ask the attention of the profession, in the most prominent manner that I could, through the medium of this Society, to two series of cases in which syphilis had been communicated to numerous individuals in the practice of vaccination. There was nothing new to medical science in the nature of the facts, for closely similar ones had occurred previously in various parts of the Continent, and had been faithfully recorded. The narratives of them had also excited much attention amongst ourselves, and had been criticised and investigated by many writers, amongst the ablest of whom I must mention the name of Dr. Ballard, who in his work on Vaccination had treated the subject exhaustively, had avowed his conviction of the reality of the occurrence, and had anticipated almost all the observations which have

occurred in connection with subsequent cases. Nor had we in English practice been wholly free from such accidents, for, not to mention some more or less suspicious occurrences which were not placed beyond doubt, a case which was perfectly clear in all its details had been brought by Mr. Thomas Smith before the Clinical Society six months prior to my own paper. Notwithstanding these circumstances, however, I believe that the facts which I brought before the Society produced in the British profession a sense of most disagreeable surprise, and awoke many for the first time to the perception of a danger which they had never before realised, and of which most had scarcely credited the existence. In the minds of some, I believe that I incurred blame, as being unwise and rash in making such facts public. I could not myself think so at the time, nor do I now, for it seemed most necessary that the knowledge of such a danger should be diffused as promptly and widely as possible. This opinion has been confirmed by the circumstance that during the last eighteen months two other examples of the communication of syphilis in vaccination have come under my notice. One of these presented itself amongst my patients at Moorfields, where the man applied on account of iritis and without the slightest idea as to what he was suffering from, and the other was brought to my house as a private patient, the lady being the subject of a urethral growth. I wish particularly to state that neither of the cases was sent to me because I had taken an interest in vaccination-syphilis, but that both showed themselves in the ordinary course of practice. It may be recollected that neither of my first series came under observation in this way, but that in both the nature of the disease had been recognised by others before I was consulted. I purpose in the present paper to relate briefly the particulars of the two cases to which I refer, and neither of which has as yet in any way been made public, and after doing this I shall ask to be allowed to make a few general remarks on the whole of the facts which I have had under observation. I may just add that during the last two years several cases have been sent

to me as supposed examples of vaccination-syphilis, but in none could the diagnosis be supported.

THIRD SERIES (ONE CASE).

The following case came under my care at Moorfields, where its subject applied on account of acute iritis, of the cause of which he had no suspicion.

The patient was a tobacconist, æt. 46, a married and respectable man. The iritis for which he applied at Moorfields was double. On investigation it was found that his body and limbs were covered with a papular and sealy rash of a very definitely syphilitic nature; he had also symmetrical sores in the tonsils. The examination further revealed the fact that he had large unhealed sores on his arm which had resulted from vaccination. He told us that he had been vaccinated three months before, being at the time in perfect health. Several of his children (young adults) were vaccinated at the same time and from the same child.

His account was that the vaccination places took and went on favorably, but that just as they were about healed, during the fourth week, they again inflamed, became ulcerated, and gave him much trouble. He attributed this relapse of inflammation to the irritation of tobacco dust, in which he worked. It was not until more than six weeks after the vaccination that the eruption showed itself, and not till the end of another month still that the iritis occurred. He had not as yet had any specific treatment, since the nature of his disease had not been recognised. On being questioned as to the possibility of his having contracted syphilis he firmly denied it. He readily submitted to an examination of his genitals, and we could not find the slightest trace of sore on them. His vaccination sores, at the time he came under my notice, were open ulcers as large as shillings, covered with scab and with dusky indurated borders. There was an indolent swelling in his armpit.

This man remained under my treatment at Moorfields

for two or three months, and was seen repeatedly by many members of the profession who were attending my clinique. There could not be the slightest doubt that he was suffering from secondary syphilis, and that too in a very severe form. We treated him with mercury, and both his iritis and rash disappeared.

Everything in the man's history that we could ascertain seemed to point to vaccination as the source of contamination. His vaccination spots were the seat of chancrous induration after having in the first instance healed. The date of their induration and the date of appearance of the rash fitted exactly with the hypothesis that he had in some way acquired syphilis about the date of his vaccination.

Having obtained from him the name of his vaccinator I called on that gentleman, and from him obtained information which strongly confirmed the man's statements. He said that he had never in his life seen such vaccination sores as this man's arm displayed. "He had been quite frightened at them," and had thought their condition so near phagedæna that on two occasions he had applied a strong solution of nitric acid. He had never, however, suspected the real nature of the disease.

About a dozen other persons had, I was informed, been vaccinated at the same time and from the same child as our patient. The vaccinator told me that he knew them all, and that, with the exception of a little trouble in the healing of the sores in one or two, none of them had shown anything peculiar. He was very anxious that we should not excite alarm in the neighbourhood by instituting any inquiry about these patients, and as he promised to get to know quietly how they were doing and to inform me if he found anything suspicious, I thought it best not to press the matter further. I ascertained from our patient himself that his own children, three in number, who had formed part of the series, had all done perfectly well. The vaccinator told me that the infant from whom he had vaccinated was a model of vigour and health, and he courtcously procured for me an opportunity of seeing it.

The Vaccinifer.—I found it a very large and very fat baby, eight months old. It had no rash nor any trace of condyloma, and the only point about it suggestive of syphilis was the state of the bridge of the nose, which was decidedly broad and sunken. Its mother appeared in good health, but she told me that her first two children had died in infancy and that this, her third, was the only one she had living.

I have, in my own mind, no doubt from the appearance of the child's nose that it is the subject of inherited taint, and shall confidently expect that future years will prove the correctness of this suggestion; nor do I see any other way in which the symptoms displayed by the man can be accounted for, excepting on the supposition that he contracted syphilis from vaccination. If we suppose that from some other source he acquired a chancre at or near the date of his vaccination, it is remarkable that no trace of such sore should have been left; and it is yet more remarkable that the vaccination spots should have become indurated and have caused an axillary bubo. Nor does it seem in the least probable that the vaccination sores had become accidentally contaminated. No plausible source of such contamination can be suggested, while, as already stated, the dates fit exactly with the supposed introduction of the virus at the time of vaccination. Thus, we seem to have an instance in which only one patient out of twelve vaccinated from the same subject became contaminated; at any rate there is, as yet, no proof that any of the others have suffered, but under the peculiar circumstances of the case I can by no means accept this absence of evidence as conclusive. It is not improbable that something more may in the future be heard of some of these patients.

I will now relate the particulars of the fourth example of vaccination-syphilis which has come under my notice. They are in most points very similar to those of the preceding case; out of three vaccinated only a single individual became diseased.

FOURTH SERIES (ONE CASE).

Mrs. M—, aged about 46, came under my care last December. Her ailment was a vascular growth in the urethra, but in the course of the examination I found that she was covered with the stains of a syphilitic rash. On my asking about it she told me that she had been very ill after vaccination and had suffered from a severe eruption and from inflammation in one eye.

The following, on inquiry, proved to be the facts of her case:—She was vaccinated in May, 1871, and her two daughters, one aged 30 and another 15, were done at the same time. Four punctures were made in her arm and all of them soon healed, not having taken; but a month later one of them again became sore, and a hard-edged very troublesome ulcer resulted which lasted for three months. Within a few weeks of the formation of this ulcer, *i. e.* within seven or eight weeks of the date of the vaccination, she became covered with rash. It was especially copious on the chest and back of the neck and affected also the palms of her hands. From the date of the vaccination in May until the beginning of September she remained under the care of her own medical attendant by whom the vaccination had been performed. In September, as the rash remained out, she consulted another practitioner. Under her second adviser she remained for nine months taking almost continuously iodide of potassium and bichloride of mercury in small doses. In July, 1872, when the rash had been out eleven months and was still but half cured, iritis occurred in the left eye. It was so severe as to threaten the destruction of the organ, and after some delay she got admitted into the Ophthalmic Hospital. It should be stated that for two months before the outbreak of the iritis she had suspended all treatment and had been staying at the sea-side for the benefit of her health. She was in Moorfields from July 19th to August 1st and was treated by mercury and atropine. The diagnosis given, as I ascer-

tained from her prescription paper, was "Iritis, specific (?), after vaccination (?)" At the time of the iritis her rash relapsed and her palms became covered with scaly patches, her finger-nails also thickened.

At the time that this patient came under my observation she was much out of health and was still covered by the stains of an eruption about the nature of which there could not be the slightest doubt. There were adhesions of the iris in the left eye. I found that she had been much annoyed with the gentleman who had vaccinated her, attributing what had followed to his having done it when she was not in a proper state of health. She had not, however, the slightest idea of the nature of the disease from which she had suffered. Her surgeon had not suspected it either, and consequently, during the first four months, no specific treatment had been adopted. On asking her as to the particulars of the vaccination she told me that it was done from arm to arm in the surgeon's own house. The infant, according to her statement, was puny and its mother looked ill. She was not aware whether the child's arm bled or not. In her two daughters who, with her, were vaccinated from the same child, no definite ill results followed; in both, several spots took, went through their stages and healed soundly at the usual time. In neither of them did any of the spots re-ulcerate, but the younger one twice suffered from transitory rash, probably urticaria. I afterwards called on the surgeon who had vaccinated this patient: he confirmed her statements in all particulars excepting that he did not allow that the vaccinifer looked puny.*

The Vaccinifer.—I succeeded in tracing the vaccinifer. Its mother was the wife of a lighterman and had borne three children previously. Of these the eldest is a boy of ten who now shows no signs of syphilis and who is said to have had no symptoms in infancy. The second was stillborn. The third was a girl of four who had no special symptoms in

* It seems probable that the woman who is the subject of our narrative was the last or last but one vaccinated out of a considerable batch.

infancy excepting that for several months during teething she had very troublesome ulcers at the anus (condylomata). For these she was long under medical care and had numerous local applications. At the same period she was considered to have "a tendency to water on the head." The vaccinifer was four months old at the date of vaccination and appeared, according to his mother's statement, to be perfectly well. Subsequently, however, when dentition commenced he, like his elder sister, had very troublesome sores at the anus and a tendency to water on the head. For the anus he was three months under treatment at a dispensary and had blue stone applied. He is living, full grown, and shows no peculiarities excepting a large forehead.*

There cannot be the slightest doubt in this case that the patient has suffered from syphilis, the only question to be raised is how was it obtained? On this point I hold that a vaccination puncture having reopened a month after healing, having formed a large hard-edged ulcer which lasted three months and which was promptly followed by the rash, is an almost conclusive piece of evidence. The dates of each occurrence are precisely what we should have expected.

The patient is a married woman past middle life, and there is not the least reason for suspecting contagion from any other source. I may add that the vaccination ulcer has left a large dusky scar exactly such as I have seen in other cases of vaccination-syphilis.

Before leaving this case I may ask attention to the fact that the secondary symptoms were unusually severe and protracted, a circumstance not improbably due to the fact

* Since this paper was written I have obtained from the mother the name and address of a surgeon who treated her child. On inquiry he tells me that the child had *syphilitic condylomata*, and was, on and off, under his care for months. He states that it had also snuffles and a slight skin rash, and that it was cachectic and puny. He did not ask the parents any direct questions because he was perfectly certain as to the nature of the ailment.

that no specific measures were employed during the first four months, and that they were subsequently given very insufficiently.

I will now, with the permission of the Society, make a few general remarks on the subject of vaccination-syphilis, chiefly in connection with the four series of facts which I have recorded. It will be convenient to arrange what I have to say under separate headings.

1st. What are we to infer from the circumstance that when syphilis is conveyed in the practice of vaccination it does not affect all of those vaccinated from the tainted source? Clearly, I think, we must believe that the specific poison of syphilis is either not contained in the vaccine lymph at all, or is not equally diffused through it. In my first series of cases two patients out of twelve were successfully vaccinated and wholly escaped syphilis; in the second series out of about twenty-six more than half escaped; and in the third only one out of twelve is known to have suffered, whilst in the fourth only one suffered and six or eight probably escaped.

In the first and second series it was repeatedly observed that of those who contracted syphilis, some of the vaccination punctures developed chancres and others did not. There cannot be the slightest doubt that it is quite easy to vaccinate from a tainted vaccinifer without conveying syphilis, and on the other hand that it is possible to convey syphilis either with or without the production of a normal vaccine vesicle. Now, the supposition that it is necessary to convey some of the cell-elements of the blood in order to convey syphilis, seems to my mind by much the most probable explanation. Probably it is not necessary that these elements should be visibly red. That the vaccine virus itself in a pure state cannot produce syphilis seems highly probable, since in several recorded instances vaccination has been inadvertently performed on a considerable scale from a child that was subsequently found to be syphilitic,

and without ill consequences. It is probable that in a great number of instances, in addition to those placed on record, this has happened, and the evidence supplied by it in reference to the impotency of pure vaccine lymph in the production of syphilis is very strong. On the other hand experiment has fully proved, and more especially the well-recorded experiment of Professor Pelpizzari, that the blood of a patient in the secondary stage of syphilis can, when inoculated, produce a chancre which will be followed by the usual role of syphilitic phenomena. The facts in the case referred to afford as regards dates, &c., a very exact parallel with what was observed in all the cases which I have recorded.

Next we may ask is it absolutely necessary that *blood* should be used in vaccination in order to convey syphilis? It seems highly probable that it is not. At any rate there is not the least evidence in three out of the four series of cases which I have recorded that the lymph used was visibly contaminated with blood. The vaccinator in each instance asserts that it is his habit most scrupulously to avoid making the vesicle bleed. Probably it is quite sufficient to allow the vesicle to draw or weep. With this drainage no doubt corpuscular elements of the blood and tissues become free. According to this supposition as soon as the first contents of the vesicle are exhausted the risk begins. It is well-known that it is the custom of many experienced vaccinators to allow the ruptured vesicle "to weep," and to continue to employ its secretion long after the exhaustion of its original elements.

If the syphilitic virus and the vaccine virus be implanted at one and the same time, what will be the course of events? The cases recorded show conclusively that, if the patient be susceptible to vaccination, the vesicle may pass through all its stages in the most characteristic manner. Then after healing of the vaccination-sore, and at the end of about a month from the inoculation, the syphilitic virus begins to show its effects and the scar becomes irritable, inflames, and indurates. Although this course is the usual one it is not

invariable and deviations from it may be observed in connexion probably with the patient's age, state of health, and condition of tissues.

In these exceptional cases the vaccination-sore never heals, and the pus-scab which forms over it combines with the inflammatory swelling around to conceal the nature of the specific changes which subsequently occur. Should the vaccination not have taken, it is usual for the puncture to heal and for the patient to think no more about it until induration occurs at the end of the month.

What are the usual characters of the vaccination-chancere ?
As already hinted above, the amount of inflammatory effusion on the surface of the sore, and of inflammatory œdema at its base, may in certain cases be considerable. In several of the cases in my second series the specific characters of the chancere were in this way quite concealed. In these instances the patients were children. In the man who is the subject of my third observation the history was that the sore had been very acutely inflamed, so much so that the surgeon several times cauterised it, and probably it was on the verge of phagedæna. These conditions are, however, exceptional and in a usual way the vaccination-chancere shows but little tendency to excess of inflammation. In some cases it does not even ulcerate. It begins as a little red, firm, glossy tubercle which gradually increases in size and becomes harder. At the end of a fortnight, or earlier, it usually ulcerates and presents a sore remarkable for its small amount of secretion and for the hardness of its base and edges. The cases in which no mercury was given show that it may last for some months before it heals. After healing it leaves a dusky brown scar very different indeed from that of vaccination.

What treatment ought the vaccination-chancere to receive ?
I can feel no doubt that should a vaccination-scar take on the induration characteristic of a chancere, and should the other facts of the case corroborate the suspicion, it is the surgeon's duty without delay to begin the administration of mercury. The cases which I have recorded show in the

strongest possible light the great difference in result between those in which mercury was given and those to which nothing was done. In my *first series* of cases the nature of the accident was recognised during the sixth week after vaccination and prior to the occurrence of any well-marked secondary symptoms. In all the patients, excepting one, mercury was at once commenced and in all these the progress of the chancre was at once arrested and rapid cure resulted. For a considerable period no secondary symptoms shewed themselves, and the success of the treatment was such as to induce not a few to doubt the correctness of the diagnosis. Subsequently, however, secondary symptoms showed themselves in several of the patients. They were so well characterised as to put all scepticism about the nature of the disease out of question, but still they were comparatively very slight. They yielded very quickly to the renewed administration of mercury and none of the patients in any material degree lost health either from the disease or the remedy, iritis did not occur in any single one and I believe they are all at the present date quite well. The only case which gave any real trouble was that of a young woman in whom suppuration in the cervical glands took place, and in her most probably it was strumous rather than syphilitic. The contrast in this respect was very great between the *first* and *second series*, and still greater in respect to the two cases which I record in the present paper (*third* and *fourth series*). In neither of the two latter was the nature of the disease suspected until the skin was covered with secondary rash. In both the chancre on the arm became very large and remained open for several months. In both the eruption came out most copiously and was attended by great loss of flesh and strength. In both iritis of a very severe character occurred. One of them was cured, both as regards local phenomena and general health, by a course of mercury; in the other the disease under inefficient treatment has lingered for twenty months and the patient is still suffering much from its effects. It is of course too

early to obtain data as to the relative liability of the patients to the tertiary forms of syphilis, but so far as the primary and secondary symptoms are concerned I cannot speak too strongly as to the vast apparent advantage of the mercurial plan. The lesson of the cases is very clearly opposed to the too prevalent modern doctrine that it is well to wait for secondary symptoms before beginning specific treatment, and would appear to indicate that the latter should be adopted as soon as ever the condition of the chancre permits of an accurate diagnosis. I may also in passing be permitted to ask attention to the interesting illustration which these cases afford of the manner in which mercury interrupts the evolution of syphilis and delays the occurrence of secondary symptoms. In all the cases which were not treated secondary symptoms showed themselves from the sixth to the ninth week after the inoculation, whilst those treated by mercury did not show symptoms until from five to seven months afterwards.

In conclusion a few words must be said as to the best means by which we may hope to prevent the occurrence of these lamentable accidents in future. Foremost under this head I would put the diffusion of the knowledge amongst the profession that such accidents are possible. Until quite lately almost the whole British profession was incredulous on this point, and in spite of the publicity which was given to the facts two years ago there still remain, I believe, many who are either uninformed or unconvinced. The vaccinator who proceeds in his duties with the fear of syphilis before him can I think incur but little risk in the matter. He will in the first place select his vaccinifer carefully, avoiding all children whose parents are not known to him. He will for the most part avoid all first-born*

* Of the vaccinifers in my four series of cases two were first-born children, the third was the first which had lived, the parents having lost two previous children in early infancy, while in the remaining case, although the child had a brother and sister living, and apparently in good health, there was the fact that the latter had in infancy long suffered from ulceration at the anus. I am bound, however, to admit that if this the second child had been presented as a

children and wait until by the development of one healthy child some guarantee of freedom from taint on the part of the parents has been given. There certainly cannot be any difficulty under ordinary circumstances in procuring vaccinifers who are absolutely free from risk. Next to the scrupulous selection of the child from whom to vaccinate come the obvious precautions of avoiding the use of blood and of recent exudation from the walls of the vesicle, but these and many other matters of detail have already been so well enforced that it is needless to allude to them further.

vaccinifer, there would, as far as can be ascertained by inquiry now, have been no facts likely to arouse the suspicion of the surgeon. The mother would have been able to produce the guarantee of an older child in good health.

A R E P O R T

ON CERTAIN CAUSES OF

DEATH IN EWES DURING AND AFTER PARTURITION,

WITH NOTES ON THE "NAVEL-ILL" IN LAMBS.

BY

JONATHAN HUTCHINSON, F.R.C.S.,
SENIOR SURGEON TO THE LONDON HOSPITAL.

Read March 8th, 1876.

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A REPORT ON CERTAIN CAUSES OF DEATH IN EWES DURING AND AFTER PARTURI- TION, WITH NOTES ON THE "NAVEL-ILL" IN LAMBS.

By JONATHAN HUTCHINSON, F.R.C.S.

SENIOR SURGEON TO THE LONDON HOSPITAL.

DURING the winter months of 1872 and 1873 I had a flock of seventy West-down ewes, which lambed during January and February. The weather was cold, but they were well protected in a large straw yard. The following is a list of our losses arranged in the order in which they occurred.

No. 1. Died a week or two after lambing, having borne two healthy lambs. The illness was obscure, and I had no opportunity of personal observation, nor was a post-mortem made. I will not, therefore, trouble the Society with any details.

No. 2. Dropped a dead and decomposed lamb, being at the time somewhat ailing. She died four days later with metritis and portions of retained placenta, and with pyæmic deposits in the lungs.

No. 3. Had a long and difficult delivery, the lambs being crossed. The hand was used to assist her, and she died a few hours afterwards. The right horn of the uterus showed a large ragged laceration, and at the fundus of the organ was another black and almost gangrenous patch. There was but little blood in the peritoneum, and no evidence of fibrinous effusion. The viscera were sound. The laceration in the uterus appeared to have been preceded by softening of an almost gangrenous character. The lambs had been dead for some time, and were softened and decomposing.

No. 4. Was a case of difficult and protracted labour. The vagina was narrow, and the shepherd failed in his attempts

to effect delivery. The ewe died undelivered ; the lambs, which were malplaced, having been dead some time previously. No injury had been inflicted on the vagina or uterus.

No. 5. Died unexpectedly one month after lambing. She was found dead on her back, having the night before seemed quite well. This might possibly have been a case of "rig-welting." The post mortem showed no disease to account for death, but the kidneys were soft and pulpy, and the cellular tissue around them blood-stained as if from bruising.

No. 6. Was a case of death on the fifth day from metritis and pyæmia. The uterus was almost gangrenous, and there was peritonitis of the adjacent parts. A portion of placenta had been retained. There were pyæmic deposits in the lungs.

No. 7. Death before delivery from rupture of the vagina and escape of a loop of intestine. The accoucheur did not admit having attempted to introduce the hand.

No. 8. This ewe died undelivered after a week's illness. She began by appearing to be dull and feeble. There were no signs of labour until the day before her death, when the waters presented and broke. At the autopsy the uterus was found to be so matted to the abdominal wall that it was difficult to separate them. Masses of lymph formation an inch thick, and looking almost like custard, lay between the peritoneal layers. The walls of the uterus in parts were discoloured and almost gangrenous. The peritonitis was clearly secondary to the metritis, for the intestines and stomachs which were pushed upwards were scarcely affected by it, being almost wholly free from adhesions. In the uterus were two full-grown lambs. Their tissues were sodden, and they had evidently been dead for a week or more. The placenta and membranes were beginning to decompose. The os was dilated and the vagina roomy, and one lamb was presenting naturally. There were no pyæmic deposits. There can, I think, be little doubt that in this instance death of the lambs from

some unknown cause was the starting-point of the mischief. Then followed the metritis and next the peritonitis. There was no reason to suspect any kind of infection. The ewe's illness began a week before parturition, when she was about in the field. The case illustrates, as do several others, the proneness of the uterus to take on gangrenous inflammation when containing dead material.

No. 9. Was an instance of death from acute bronchitis of two days duration six weeks after lambing. I have full notes of the illness and post-mortem, the latter done by Mr. Nettleship ; but as it is not specially connected with parturition I forebear to quote them.

It will be seen that during this year my mortality reached the high average of nearly 12 per cent. Yet we had no epidemic and nothing in the season specially prejudicial. The deaths were from very various causes. We may put aside Nos. 5 and 9 as having had nothing to do with parturition, and case 1, as having not been diagnosed. Of the remaining 6 two died during delivery, one with vaginal injury, and the other from exhaustion, delivery being impracticable ; neither having any inflammation. This leaves 4 in which inflammatory changes ensued, and in all of these metritis from decomposing placenta was the first change. In one the almost gangrenous organ had ruptured, in another there was extensive and severe pelvic peritonitis, and in the remaining 2 in addition to metritis and peritonitis there were evidences of pyæmia. In most of these latter cases the lambs had been dead some time. Difficulty in the process of parturition then appears to have been this winter the primary cause of loss. Consequent on it, death of lambs in the uterus was liable to happen, and as a sequence of this came metritis and other inflammatory changes. The suspicion arises that this high ratio of difficult labours may have been incidental to the breed, and to the attempt to obtain large animals. The West-down sheep is a cross between the Leicester and South-down, and is much larger than the latter. It may be that the average lamb is out of proportion to the vagina. It

seems, however, highly probable that some other cause of death of lambs was at work, for in several instances the death occurred without any signs of labour being present.

During the next spring (1874) I had 40 ewes of the same breed (not the same animals). Of these 8 proved barren, 2 lost their lambs, and 2 died. The remaining 28 reared 34 lambs. No death occurred this year in direct connexion with parturition.

One ewe (No. 11) died after an illness of three days before labour commenced. Her lambs were living and active up to the day before her death. She had seemed weak, and during the last day had lain on her side with her head bent back (? tetanic) (I did not myself see her). After death the only post mortem condition of importance found was universal hepatisation of both lungs. This was almost literally universal, and the lung substance could hardly be distinguished from liver. This must I suppose rank as a death from double pneumonia.

A second ewe died several weeks after lambing from septicæmia consequent on gangrenous inflammation of the udder. The shepherd drew my attention to the fact that the udder was swollen and hard. I found it very large, dusky, and with fluctuation in one part. A free incision was made, and a quantity of thin gruel-like pus with flakes of milk was let out. The inflammation spread to the cellular tissue of the abdominal wall, and on the next day death took place. At the time I made the incision the shepherd remarked that he had never known recovery from a state like that. The condition is known as "black udder." It is evidently inflammation of the breast gland in the first instance, the peculiarity in the sheep being that instead of the process ending in the formation of circumscribed abscess it becomes diffused and rapidly runs on towards gangrene. The animal usually dies either of shock or of septicæmia before absolute sphacelus is produced. Two or three days are, I believe, the usual limit of the illness. When a student I saw and dissected several such cases, and they occur, I believe, in the cow as well as the

ewe. It may be that the depending position of the udder in these animals aggravates the inflammatory process, but no one can believe that this is all. The rapid manner in which all the adjacent tissues become blood-stained and livid is quite peculiar to the lower animals. I have in women seen very severe examples of mammary inflammation, and one at least which threatened to end fatally from excessive constitutional disturbance. I never, however, saw or heard of one which really ended in death at this stage.

My flock in 1874 and 1875 consisted of 50 horned Dorset ewes to lamb in October and November. We did not lose any from causes connected with parturition.

During the past winter 1875 and 1876 I have had many deaths, and have to record some very interesting facts respecting them. We bought in November 100 horned Dorsets to lamb at once, but the lambing season as it turned out was protracted over three months. The season was bad both as regards wet and cold.

The first death occurred on November 13th, three days after delivery. The uterus was swollen, soddened, and discoloured; portions of its peritoneal surface were greenish and showed shreds of lymph, but there was no general peritonitis. The uterus contained a dirty grumous fluid, but there were no portions of placental structure of any material size. All the other viscera were healthy excepting that the left lung was everywhere adherent, but the adhesions were certainly not recent.

Here, then, we have proof of a fatal metritis immediately after delivery without obvious cause, occurring under circumstances which put the risk of contagion almost entirely aside. It was the first death of the season, and the animal had been delivered in an open shed.

On November 15th another ewe died, as was reported with precisely similar symptoms, but it was not practicable to make a post mortem. About the same time another ewe was ill with a dirty discharge from the vagina, but after a fortnight's illness she recovered.

We have now to enter upon a wholly different type of disease.

On November 19th two ewes were reported to be ill. When I saw them on Saturday the 20th one was evidently very ill. Her flanks projected as if blown up, the right side being especially conspicuous. The shepherd wanted me to puncture the flank, but I could not make out that there was any fluid. The swelling seemed rather to be caused by retraction of the abdomen. This ewe had lambed about nine days ago, and her milk still flowed fairly. We noticed that she had a little yellow purulent discharge from the vagina. She was in the open field, and the weather was slightly frosty. At this time she could walk about, but she had a heavy dazed look about the eyes. During the course of the day she became rapidly worse, and in the evening was thought to be dead. During the whole of the night she lay on her side quite rigid, and excepting respiration quite motionless. If she had been made of wood or well frozen she could not have been more stiff. Her limbs, back, neck, and jaw were all equally rigid. By taking hold of one leg you might lift her body and turn her so as to make her rest on the rump and back of head. There was no great opisthotonos; it was simply absolute rigidity with the neck moderately bent backwards. The jaws could not be opened excepting by force which seemed likely to break them. On Sunday evening, as she had been for twenty-four hours in this condition and there seemed no probability that she would improve, I had her killed in order that I might make the post-mortem before returning to town. As regards its details I may briefly say that we found nothing. The viscera were all quite healthy, including the uterus, which was contracted, and did not show a trace of inflammation. The udder was full of milk, and although a small quantity of puro-mucus was found in the vagina there was nothing of any moment. I did not in this instance examine the spinal cord and brain, but as in subsequent quite similar cases I repeatedly did so I think we may safely assume that they would have presented no morbid conditions.

The other ewe which has been mentioned as having

sickened on the same day as the one just referred to, was one which had lambed the first in the flock, had lost her lamb, and had since seemed perfectly well. She was in excellent condition and the udder quite dry. On the Saturday morning the men noticed that she staggered and looked what they called "fitty." I could see nothing amiss with her excepting, perhaps, that her eyes looked a little dull, but if driven she reeled a little in her gait, her pupils acted well and she still ate freely. Next morning, however, she was worse, could not use her limbs and lay sprawling in any position in which she was put. Her jaws were however, still not fixed and she could eat. On the Monday morning (third day of the illness) she was worse and in much the same condition as I have described in the preceding case, with the exception that her jaw was not nearly so stiff. Her neck was thrown back just as in the other case, and when placed on her back she rested on the back of head and rump. With a little force, however, her jaws might be opened and if food were put into her mouth she could masticate and swallow it. Although thus apparently about to die on the Monday she yet lived on till the Friday evening, the men being very attentive to feeding her. She appeared to die from inanition, for the quantity of food taken had been but small. The post-mortem was made on Sunday, November 29th, about thirty hours after death. The tissues were already although the weather was cold, much softened and discoloured. No special morbid conditions were found, the uterus was quite healthy, the liver and kidneys shared with the other tissues in softness, but showed no disease. The brain and cord were carefully examined and were quite healthy.

Thus, then, we have two deaths at periods respectively of ten days and nearly a fortnight after lambing, with similar symptoms, and in each instance without uterine lesions, whilst in each the disease appeared to be a form of tetanus. I shall have at a subsequent part of the paper to mention a number of similar cases and also to assign both cause and cure. As I wish, however, to still observe chronolo-

gical order in my narration, I must for a little space leave this subject to relate a case of death in connection with the labour process.

On Sunday, November 21st, an ewe showed her first symptom of labour in a considerable prolapse of the rectum. Although repeatedly reduced, the prolapse continued to recur on Monday, and the men then, according to custom, stitched up the anus. On Tuesday the stitches were cut, and the prolapse never recurred. On Wednesday she was so much exhausted that it was necessary to interfere, and with much difficulty a very large lamb, which had apparently been dead a week, was taken away. It was quite rotten. The mother was very ill at the time, and died the next day. I found at the autopsy the vagina much bruised and its mucous membrane lacerated. It was, indeed, almost gangrenous. In the upper part lay a second dead lamb, a small one and partially decomposed. It had probably been dead ten days or a fortnight. The placenta of the first was still retained. The uterus did not show any material signs of inflammation; its interior was almost dry and its walls thin and of a livid colour; on its peritoneal aspect were a few flakes of lymph, and half a pint or more of dirty blood-stained fluid was in the peritoneal cavity. There were no evidences of peritonitis excepting in the neighbourhood of the uterus. The lungs were almost everywhere solid and liver-like. There were no deposits in them and no pleuritis.

In this case there is little doubt that death of the lambs at separate periods occurred before parturition commenced, and that metritic peritonitis was the cause of death, helped, probably, by the introduction of the hand, &c., to procure delivery.

On December 5th another ewe was reported "stiff." She had lambed two weeks before, but had never seemed quite well afterwards. Her own lamb was dead-born, and attempts were made to get her to adopt another. With this object she was kept in a hurdle shed from the time of her lambing until she sickened with tetanus. I mention

this fact in order to make it clear that the latter was not produced by exposure to cold. I need not again describe the tetanic symptoms, but may say that this sheep was just like the two others, excepting that she had in addition spasms when touched. These spasms were not very severe, but they were constantly produced by touching her.

In this case the tetanic condition continued for several days before death occurred. A careful post-mortem, including the brain and spinal cord, disclosed nothing.

About this time several other sheep began to present similar symptoms. Some after having seemed likely to sicken rallied again and got well, but others passed into conditions very similar to those described. I now began the administration of opium in full doses, and for some time it appeared to be able to control the conditions, even when most severe, and we had no more deaths. The dose given was eight grains of powdered opium in a little gruel, and this was repeated every three hours if necessary.

On December 7th my bailiff reported that he had cured three with the opium, but had lost one, which, indeed, had died so quickly that no medicine could be given. It was only noticed for the first time to be ill when the shepherd visited them at six in the morning, and at eight it was dead. This ewe had only lambed a few days and had never left the shed, so that the malady could not be attributed to cold. She died quite rigid, and with the jaws firmly closed. This case, in which the illness was certainly not more than twelve hours, was the shortest that we had.

On January 1st another ewe died very shortly after delivery by the hand. She had twin lambs, one of which lived almost to delivery, whilst the other had been dead some time and was decomposed. No post-mortem.

I must now conclude my account of the tetanus epidemic. During January we still continued to have many ill. In most the opiate treatment succeeded like a charm, but still a few died. In one case, an ewe which was lying quite stiff, and with the jaws so closed that it was only

with the utmost force that they were got open to give the medicine, was walking about and calling for her lamb within an hour or two of the second dose. We did not now lose any excepting those which died very quickly; if the disease was protracted the opium cured it. The number of cases, however, still increased. I was exceedingly puzzled to assign any cause for this malady. None of the flocks of the farmers in the neighbourhood were suffering, nor could I gain any information about it from my friends or from books. It is a well-recognised fact that sheep and other ruminants, are very liable to tetanus, but no one has, I believe, recorded any statements bearing upon the special liability of the post-puerperal state. It was impossible to blame the cold, for the cases had been equally numerous during some very warm weather, nor even during the cold had the sheep endured any exposure. They had been most carefully attended to both as regards housing and food. It is to be added that we had tried repeated changes of field and had divided them into two flocks, keeping one a quarter of a mile apart from the other, but with the result that the cases still occurred alike in both. At length in the last week of January we determined to make a change in the kind of cake they were having. In the beginning of the winter they had had a mixture of linseed cake and cotton-seed cake, but latterly they had had cotton-cake alone. The cases had increased since the linseed cake had been finished. The result was that after finally forbidding the cotton-cake no more cases of tetanus occurred. There can, I think, be no reasonable doubt that it was the cause, for up to the time of its abandonment they were increasing in frequency, with its disuse they ceased. I fear that some members of our Society may complain that I have troubled them with a long account of a malady which, after all, turns out to be not a puerperal one.

It is, however, to be remembered that, although it is true that the immediate and chief cause seemed to be the cake, yet none were attacked excepting those which had

lambled. None of the lambs, although they ate the cake freely, ever suffered, nor did any of the ewes before lambing, nor any of those which were not in lamb. Thus it seemed certain that there was something either in the fact of recent delivery, or in the lactation state, which materially predisposed to it. This may probably in part be explained by recollecting that those ewes which were giving milk would be the most likely to eat freely of the cake. The cake was a very good one, and is considered equal in all respects to linseed, and the rest of the farm stock, steers, calves, &c., as well as the lambs, were doing well upon it.

After I had given it up I learnt from several farming friends that they considered it a doubtful food for ewes, but I have not yet learned from any source that it has been considered chargeable with producing tetanus. I do not wish to trouble the Society with any merely agricultural details, and must therefore be content to ask attention to the remarkable fact which this series of cases seems to illustrate—that it is possible, by an article of food not in itself actually poisonous, to predispose so efficiently to so peculiar a form of nerve disorder. It seemed clear that it was not poisoning in the more direct and ordinary sense of the term; that is, it probably did not depend upon any accidental ingredient present in some parts and not in others. Had it been analogous to strychnia poisoning for instance, it is probable that if the animal lived on for a few days after the beginning of symptoms and after ceasing to take the cake, recovery would have ensued. But this was not the case, for in all instances in which the disease became severe, unless opium was given the animal died, whilst in some the illness was protracted over a week. From first to last I do not think that less than thirty out of the flock suffered, and of these eight or nine died. Although all books on the diseases of sheep acknowledge that they are very liable to tetanus, yet I cannot learn from private inquiry that it is at all frequent. A gentleman who has been a considerable sheep-breeder the last twenty-five years

tells me that he has never seen but one sheep with "locked jaw," and that was after an operation. I have myself seen it once in a goat after an injury to the foot. It is to be observed that, in the series which I have described, the general tetanic symptoms were never absent, whilst in several cases the local ones of trismus were comparatively slight. So also the tendency to spasm was but rarely present, and was never so well marked as it often is in the human subject. That there was no organic disease was proved not only by the negative results of the post-mortem examinations, but by the remarkable manner in which some of the worst cases recovered in a few hours under the use of opium. I do not know that any of those which had once recovered relapsed, but this is not certain.

I wish next to ask the Society's attention for a few minutes to a peculiar disease to which young lambs are liable. I do not desire to say anything on the causes of death generally, although there are many interesting points which might be mentioned, but for this evening it may perhaps be better to keep to one only.

Shepherds are well aware that lambs are liable to suffer from what they call "navel-ill," and some veterinary works, Mr. Gamgee's for instance, mention pyæmia as an occasional result of inflammation of the funis. Mr. Gamgee suggests that it may perhaps be caused sometimes by the practice of wrapping a lamb in the skin of one which has died in order to induce the mother to replace her loss by a foster-child. My experience this spring has, I am sorry to say, demonstrated in the strongest possible manner that no such contamination is needed in order to produce purulent inflammation of the funis and pyæmic hepatitis. I have lost at least a dozen lambs from this cause, and in none of them can I suggest any possible source of local poisoning. In none of them had the practice adverted to been employed, and in only a few had the funis been in anyway touched by the hand of the shepherd. In many cases, in the ewe, the umbilical cord is torn across after delivery by the mother's rising, and then I believe it usually

breaks about four or five inches from the lamb's body. In other cases the ewe bites it across, and in some instances, even after it is torn, the ewe, after licking her lamb, will proceed to nibble off the funis close. If the shepherd is near at the time of the lamb's birth he usually cuts the cord with his knife, leaving about four inches attached. Under no circumstances is any ligature applied, and external hæmorrhage is, I believe, unknown. Shepherds have a prejudice as to the ewe's biting the cord too close, and think that the navel is more liable to inflame when this is done.

The first case of navel-ill was one in which this had happened. The cord had been bitten close, and the parts about the umbilicus were swollen and discoloured. The lamb died at about a week old. I found the cellular tissue of the abdominal wall infiltrated with blood-stained serum, and the appearances suggested that there might have been some hæmorrhage, although not much. On seeking the hypogastric arteries I found to my astonishment that their ends had retracted far within the abdominal cavity, and lay close by the sides of the bladder. They were completely sealed and there was no blood-clot about them, though the cellular tissue was stained. My impression at the time was that this lamb must have died from the too-close biting of the cord, the great retraction of the arteries, and bleeding into the cellular tissue. Subsequent dissections, however, showed that this condition of retracted arteries was the natural one, and I found them in precisely the same position and condition in cases where two or three inches of shrivelled funis still hung at the lamb's navel. These later *post-mortems* showed me that in most instances there was no indication of any hæmorrhage whatever, and the almost invariable presence of pyæmic deposits in the liver revealed the true nature of the disease, and led to an examination of the umbilical veins. In several I was able to demonstrate that this vein contained pus, and in one it was dilated into a sort of abscess cavity. In others, although the deposits in the liver were just as definite, I was not able to prove phlebitis at the navel. The deposits

in the liver were always multiple, and usually placed abundantly on its surface. Sometimes they were abruptly limited to the left lobe—a condition to be explained no doubt by the anatomical distribution of the vein implicated.

In some cases there was inflammation over the surface of the liver, or even general peritonitis, and in one or two pleurisy, pneumonia, and joint complications were present. Thus there could be no sort of doubt as to the nature of the malady, and there could be equally little that it originated in connection with the umbilical vein. No such condition has been observed, I believe, in the human subject, nor am I aware that it has been described in any other of the lower animals.

It will be seen that the anatomical relations of the contaminating veins are different from what is usual in man in most cases of pyæmia since the vein breaks up into smaller branches before communicating with the general circulation. Thus, the liver is almost certain to suffer first and very severely, since its capillaries will serve as a sort of filter to detain the infecting material. It is only when in the human subject some part of the portal vein system is implicated that the conditions are parallel.

In this circumstance we have no doubt an explanation both of the severity with which the liver usually suffered, and of the fact that it was usually the only viscus affected.

As to the causes which induce the phlebitis I can scarcely offer any conjecture. I am not aware that I have lost any lambs from it until this winter, and there have been really no unusual conditions in their management which can be mentioned. It happened to vigorous lambs as well as to weakly ones, and to those in which the funis was left long as well as to those in which it was bitten off close. The external evidence of inflammation of the navel was often very slight, and in some wholly absent. In some instances the lambs lived for a fortnight, but in most death took place within the week.

On Sunday, November 29th, I had four lambs on the table

at once all presenting similar conditions ; two of them were very fine ones and had not seemed to be ill till a day or two before they died. The weather during the preceding fortnight had been cold, but not remarkably wet. It is difficult not to suspect that some source of contagion may have been present, but it is equally difficult to form any plausible conjecture as to what it could have been. It is to be kept in mind that this little epidemic of navel-ill occurred whilst the ewes were suffering from tetanus, and when we had scarcely any cases of metritis up to the navel.

Before concluding a paper which has I fear been found rather tediously matter-of-fact it may perhaps be convenient to point out what seem to be the special features of interest. These may be enumerated as follows :

1st. It seems clear that the death of the foetus before delivery, is a far more serious occurrence in sheep than in the human subject, and also that it is far from an unfrequent one. The lamb after death appears to become not unfrequently a cause of metritis, and there seems to be but little tendency to its expulsion.

2nd. When metritis occurs in the ewe, whether from a retained dead lamb, a retained portion of placenta, or simply as the sequence of parturition, it is apt to run a rapid course. The tissues of the organ soon become infiltrated and sodden, and there is a tendency to gangrene during which rupture of the walls may take place.

3rd. In connection with uterine inflammation, whether after delivery or after death of the foetus, peritonitis and pyæmia will not unfrequently occur.

4th. There appears but little reason, so far as my experience has gone, to believe that puerperal metritis and peritonitis, or the consequent pyæmia, are other than "accidents." In all cases that have come under my observation I believe that the disease was self-developed. We never once had reason to suspect contagion.

5th. It is conclusively proved that puerperal peritonitis

and pyæmia may occur in animals treated in the open air, and yet run a course almost precisely similar to the parallel maladies in the human subject.

6th. We have seen that ewes, whilst giving milk, may be made liable by an improper article of food, to a sort of idiopathic tetanus; and further that the same article of food shows no tendency to produce it in lambs, hoggets, and undelivered ewes.

7th. Lastly, we have seen that young lambs are liable, quite independently of any known source of contamination, to the occurrence of purulent phlebitis of the umbilical vein with the consequent phenomena of pyæmia. The strong support which this last fact gives to what I believe to be the true hypothesis of all pyæmia, strictly so called, is so far as I am myself concerned an abundant reward for the trouble which has attended the collection of the facts detailed. I can only hope that the Fellows of the Obstetrical Society may have found in this or other parts of my narrative that which may seem in some measure to justify me in having brought it under their notice.

NOTES OF A SECOND CASE
OF
ABDOMINAL SECTION
FOR
INTUSSUSCEPTION INTO THE COLON,
WITH REMARKS ON THE DETAILS OF THE
OPERATION.

BY
JONATHAN HUTCHINSON, F.R.C.S.,
SENIOR SURGEON TO THE LONDON HOSPITAL; SURGEON TO THE ROYAL LONDON
OPHTHALMIC HOSPITAL, MOORFIELDS; AND TO THE HOSPITAL
FOR DISEASES OF THE SKIN.

Read December 14th, 1875.

[*From Volume LIX of the 'Medico-Chirurgical Transactions,' published
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1876.

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HOSPITAL FOR DISEASES OF THE SKIN.

Received December 1st—Read December 14th, 1875.

A SECOND case of operation for intussusception, which occurred to me a few weeks ago, gave me the opportunity of acquiring additional experience as regards some points of detail which it is, I think, of importance to bring before the profession.

The patient was an infant, aged 6 months, under the care of Dr. Madge, by whom I was asked to see the case on the fourth day of the illness. The symptoms had from the first been well-marked, the child having experienced frequent attacks of painful straining with the passage of bloody mucus. The invaginated part could be easily felt in the left flank by manipulation through the

abdominal wall, and also by the finger introduced into the rectum. It had not been actually extruded, but its lower end was often within an inch of the anus. Dr. Madge had fully diagnosed the case before I saw it, and the usual measures of treatment by injection, &c., had been carefully employed. An infant sister of the child had died of the same lesion about a year previously. When I saw the infant it was evident that there was no time to be lost, for it had rejected all food for three days. It was very weak and did not seem likely to live many hours; we therefore persuaded the parents to allow us to perform the operation at once. I found the operation, as perhaps might be expected at so early an age, much more difficult of accomplishment than in my former case, and it is chiefly to these difficulties that I wish to ask the attention of the Society. The abdominal wall was thick, being loaded with fat, and the space between the umbilicus and pubes was very short. Having made an incision in the *linea alba*, just sufficient to admit two fingers, I was able easily to reach the neck of the intussusception, but I found it so slightly moveable that I could not by any means hook it into the wound. I was very anxious not to allow the intestines to escape through the wound, and finding that I could draw the bowel out to a certain extent between the tips of my two fingers, I spent some time in trying to accomplish reduction in this manner, hoping to complete it without exposing the bowel to view. At length, however, I became convinced that this was impracticable; only a certain length could be drawn out and then it became firmly fixed. I was obliged, therefore, to enlarge the wound freely above the umbilicus, and to allow the intestines, much distended with gas, to escape. It was only when the abdomen was almost empty that I could bring the neck of the sac into view in the wound, and I then made repeated attempts to draw the bowel out, but without success. That there were no adhesions was proved by the fact that an inch or two could be easily drawn out; the impediment was clearly due to the

ensheathing bowel being thrown into folds by traction, and thus constituting a series of strictures which gripped its contents. In this dilemma, and when almost in despair as to whether I should accomplish the reduction, it occurred to me to seek the lower end of the invaginated part, and try to hold the ensheathing layer so as to prevent its being drawn into folds. This led to the discovery that it was only the neck of the invaginated tract that was in any degree fixed (by its mesentery); the lower part, consisting of the sigmoid flexure of the colon much elongated, lay in loose folds on the rim of the pelvis, and was hooked out of the wound with the greatest ease. The attempt to hold the ensheathing layer straight at once revealed the true method of reduction, for by pulling this downwards, instead of trying to pull the involved part upwards, I accomplished the replacement with the greatest ease. The *appendix vermiformis* came out last, just as the reduction was completed, proving that the intussusception had begun at the cæcum. The operation had been protracted and the infant was almost pulseless, but there still remained the task of replacing the intestines within the abdominal cavity. I had some difficulty in accomplishing this, and several times after their return, portions escaped again before I could close the abdominal wound. Under these circumstances I was induced to prick the distended small intestine at two or three places with a harelip-pin to allow the escape of flatus. To my chagrin, however, just as I was closing the abdominal wound I observed that one of the punctures in the bowel was bubbling, and that there was a stain of feculent matter on the peritoneal surface; this was carefully wiped away and the wound closed. The child, although in extreme collapse when the operation concluded, rallied subsequently, became warm, took the breast, and passed a feculent motion. Death, however, took place in the night, about eight hours after the operation.

At the *post-mortem* next day we found no trace of any

further escape of fæces, but there was almost universal peritonitis, the coils of intestine being glued together by lymph. It became of interest, as one of the child's sisters had died with precisely the same condition, to ascertain whether there was any congenital peculiarity which might account for its occurrence. We found the parietal peritoneum in the right flank quite smooth and entire, whilst the cæcum hung very loosely attached by a long mesentery. This had probably been much stretched in the process of invagination, but there could I think be no doubt that the cæcum had been loose congenitally. No reproduction of the invagination had occurred.

It is my impression, judging from my experience of this case, that in future operations the lower end of the invaginated tract ought always to be first sought, and that reduction ought to be accomplished by squeezing it or pulling the sheath downwards rather than by attempting to pull the contained tube out. I am not at all sure that in some cases this might not be accomplished without bringing the parts into view; should this not be found practicable, however, it is probable that the operator will find it much more easy, in cases of intussusception into the descending colon, to bring the lower part into the wound than the upper one. It is the more necessary to draw attention to these points because they are both probably unlikely to occur of themselves to the operator's mind.

ON
HIGH AMPUTATION FOR SENILE
GANGRENE.

BY

JONATHAN HUTCHINSON, F.R.S.,
EMERITUS PROFESSOR OF SURGERY TO THE LONDON HOSPITAL, AND
PRESIDENT OF THE OPHTHALMOLOGICAL SOCIETY.

Read December 11th, 1883.

*[From Vol. LXVII of the 'Medico-Chirurgical Transactions,' published
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ON
HIGH AMPUTATION FOR SENILE
GANGRENE.

BY
JONATHAN HUTCHINSON, F.R.S.,
EMERITUS PROFESSOR OF SURGERY TO THE LONDON HOSPITAL, AND
PRESIDENT OF THE OPHTHALMOLOGICAL SOCIETY.

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My wish in writing the present paper is to claim the attention of the Society to the advantages of amputation high up in all suitable cases of senile gangrene. The practice of amputation for this disease has I believe been almost universally discouraged from the belief that it is generally followed by sloughing of the stump. I wish to urge that this occurs only when the part is removed too near to the disease. If done low down then I fully admit that the condition of the vessels will rarely be found to be such as to admit of repair, and that gangrene of the stump will usually occur immediately, and place the patient's life in much more danger than before.

As regards these operations I have had but little experience, having long believed that they were most inadvisable. The few trials that I have made have fully confirmed this opinion, and I would abstain as scrupulously

in senile gangrene as in frost-bite, from any interference with the knife near to the dying part. If, however, we go much higher up and amputate through parts which are still well supplied with blood the results are quite different.

By operations of this kind I believe I have repeatedly saved the patient's life. By "amputation high up," I mean in the case of gangrene of the foot, above the knee, and in that of the hand at or near the shoulder-joint. By senile gangrene I mean chiefly gangrene in connection with calcification of arteries, but I wish to be understood to include all cognate conditions, that is, all in which the death of the part occurs in connection with slowly progressive interference with the supply of blood. In the strictly senile forms this interference is usually greatest in the distal part of the arterial system, and it is of a nature to be steadily on the increase. Hence the hopelessness of improvement and the great danger of advance. It is impossible that any collateral circulation can be established.

In cases of gangrene from occlusion of the main trunk without disease of the smaller branches, as for instance from embolism or from ligature, the conditions are different. Here there is hope that, when the collateral circulation shall have become established, repair will take place with exfoliation of the sphacelated parts. Yet there are some cases, even of this kind, for which I think high amputation may become advisable. Certainly, amputation near to the gangrenous part should always be avoided unless, indeed, when after repair has considerably advanced there is proof of well-restored circulation. Cases of frost-gangrene stand in some sense midway between these two classes; they are examples of gangrene from distal suspension of circulation as in the senile cases, but the cause is not, as in the latter, a permanent one. There is hope of restoration of the supply of blood. Nevertheless, all authorities are, I think, agreed that this restoration is never sufficiently perfect to make it wise to attempt the removal of the affected parts,

and that it is necessary to leave this almost absolutely to natural processes.

Before proceeding to narrate my cases it will be well that I should here introduce a brief summary of what has been done by others in the same direction.

The proposal to amputate high up in cases of senile gangrene is not absolutely novel. In fact a discussion which bore closely upon it took place in this room more than thirty years ago. On that occasion the late Mr. Garlike of Rickmansworth brought forward a case¹ in which he had amputated in the upper part of the thigh, in a man aged 69, who suffered from senile gangrene in his foot. There was ossification of the femoral artery, and there could be no doubt as to the real nature of the gangrene. The case, however, did not imply any proposal to amputate high up as the best treatment for gangrene of this kind, since it was only determined on after suppurative destruction of the knee-joint, when there was no longer any choice as to the site of the operation, and could be but little difference of opinion as to its absolute necessity.

The case, notwithstanding, is a good example of complete recovery under the somewhat desperate conditions referred to.

In the discussion which followed, Mr. John Adams, my late colleague at the London Hospital, mentioned a case of recovery after amputation for senile gangrene, but the precise part at which the amputation was done is not specified. Mr. Fergusson mentioned that he had operated in a single case, but without success, and spoke with decided disparagement of the practice. To Mr. James, of Exeter, belongs, I believe, the credit of having definitely proposed the line of practice which I shall endeavour to support. Indeed, I believe in one case that he actually did what I only proposed to do, but was not allowed by my patient to carry out, viz. to amputate both the lower extremities. I cannot find that any surgeon, writing since this discussion took place, has recom-

¹ 'Medical Times and Gazette,' 1853, vol. i.

mended the practice, or even thought it worthy of serious consideration.

Even before the date referred to, Mr. Langstaffe and Mr. Guthrie had each of them in single cases amputated above the knee for senile gangrene of the foot. Mr. Langstaffe's patient died in consequence of the hæmorrhage within twenty-four hours, but in Mr. Guthrie's case the stump was almost healed when death from exhaustion occurred. Mr. Mott had, I believe, also done a single operation of the same kind.

Thanks to modern improvements in the details of operations and the management of wounds, the surgeon of to-day approaches the subject with great advantages.

As regards the risk of hæmorrhage during the operation the use of Esmarch's bandage unquestionably gives much real assistance. The calcified arteries, even those of the smallest size, are very easily found, and if tied with their accompanying veins in one loop and with a moderately thick ligature, I believe no difficulty will be found in making them secure.

Some years ago I had a case in the hospital in which the conditions causing the gangrene were complicated and in which the safety and advantages of high amputation were very strikingly exemplified. The patient was a strong-looking man of not more than thirty who had served as a soldier in Canada. There he had suffered from frost-bite of the left foot. It was not, however, followed by immediate gangrene, and a year later he came into the London Hospital with an ulcer on the foot which had destroyed several toes, which would not heal, and which every now and then showed a tendency to spread at its edges by fresh sloughing.

After he had been some time under observation my colleague, Mr. Little, during the summer vacation amputated through the tarsus. When I resumed duty I found the face of the stump gangrenous and the condition of things worse than before. It was now discovered that the femoral artery in the thigh was obliterated. I decided to

amputate again and to do what I then considered a high operation. The leg from the ankle upwards was perfectly healthy, and I amputated just below the knee. The result, however, was that the gangrene attacked the stump, and this time it was an acute form and rapidly spread upwards. The man became exceedingly ill, sick, and delirious, and in this condition I amputated again high up in the thigh and with the most happy results. All the septicæmic symptoms at once subsided, and the stump subsequently healed perfectly.

It occurs to me as very possible that in this case the occlusion of the artery may have been the unsuspected cause of spreading gangrene which took place soon after the frost-bite. My next case seems to be an important item of evidence in this direction.

A man, æt. about 65, received a slight burn in the left hand. The result was spreading gangrene of the fingers, for which he came into the London Hospital. A large part of the hand was then black and sphacelated. We found that he had no pulse at the wrist and that his brachial artery was a solid thick cord. In the axillary, pulsation could be felt. During the next ten days the occlusion extended higher up and involved the axillary. The old man was very ill and clearly about to die if not relieved. I amputated just below the shoulder-joint and found the main artery completely plugged by a dense firm clot. The smaller branches were, however, pervious and bled freely. We had some difficulty with the wound, the edges of which sloughed a little. Ultimately, however, he recovered perfectly. In this case the arteries were not calcified, but very greatly enlarged and thickened. In the opposite limb the brachial artery was visible through the skin as a sinuous pulsating cord down the upper arm and in front of the elbow.

Encouraged by the result in this case, and about the same time, I amputated above the knee for an old gentleman living in Camberwell, but unsuccessfully. He sank within a week with gangrene of the stump. In this

instance the operation was done under most unfavorable circumstances and as a last chance. The patient was stout, in bad health, and the gangrene extensive and advancing. It was of the moist form, and the conditions were so urgent that I did the operation within a day or two of my first seeing the patient.

My next case was one of an ordinary form of senile gangrene of the right foot in a thin old man, æt. 72, in the London Hospital. He suffered also from prostatic retention of urine. Amputation above the knee was followed by good results so far as relief of pain and almost complete healing of the stump were concerned. The patient lived about five months afterwards, but never left his bed. He died ultimately from exhaustion by various causes, and with a gangrenous patch on the other foot. His arteries were very extensively calcareous.

In a fifth case I amputated for spreading senile gangrene of the left foot in an old woman, aged 75. She was suffering severely from the pain and irritation; was almost constantly delirious, and certainly not likely to live more than a week when the operation was done. She was, however, fortunately very thin. After the amputation she did well without an interruption. Her stump healed without a drop of pus and became perfectly sound. Her circulation was so feeble that we had great difficulty in preventing gangrene in the toes of the other foot, and had to keep it wrapped constantly in cotton wool. She returned home about a month after the amputation in tolerable health, but with the other foot still in a condition to cause great anxiety. I had advised her to have that limb removed also, as the best means of making the conclusion of her life comfortable and enabling her to leave her bed. It seemed certain that the toes would at once pass into gangrene if she were to sit up. My advice on this point was, however, declined by her friends. The removal of both lower extremities seemed to them a mutilation too formidable to be considered, and they took her out of the hospital.

The last case which I shall mention is one of unusual interest. The gangrene was senile in the sense that it was caused by calcareous arteries, but the patient was not advanced in years. He was much out of health from other causes, and the gangrenous process was preceded and attended by inflammatory action.

I attended Mr. H— in consultation with Mr. Linton Brunton, of Limehouse, whose relative he was. Mr. H— was only forty-eight, but he had worked hard and lived freely, and although not showing other signs of old age his arterial system had become most extensively calcareous. His pulse at the wrist was most feeble, so much so that frequently for days together it was scarcely perceptible. He was liable to become blue in the face on exposure to cold air, and suffered much from cold extremities. He had once at a railway station fallen unconscious, probably from cardiac syncope, and once or twice after mental excitement had been so prostrate that he was only kept alive by the freest use of brandy. Such being the state of his circulation, he was exposed one snowy day in 1880 to cold, and came home with his feet much chilled. Bullæ formed on his left foot and were followed by gangrenous patches of the skin and much swelling. The bones seemed to suffer more extensively than the skin, and abscesses subsequently formed about them. This might perhaps be explained to some extent by the fact that there was always a certain quantity of sugar in his urine. During the two months that I watched the case Mr. Brunton and I removed piecemeal many of the digital phalanges. Gradually the disease spread up the foot; abscesses with gangrenous patches of skin formed, and the ankle-joint became affected.

We obtained the advantage of a consultation with Dr. Mahomed on December 24th, 1880, concerning the state of the heart, &c. Dr. Mahomed used the sphygmograph and gave me a detailed written report. It was to the effect that there was no proof of arterial disease in the upper extremities, nor any of valvular disease

of the heart ; the latter organ was believed to be feeble, possibly dilated, and probably fatty. The pulse was described as "extremely small, non-persistent, readily compressed, short ; artery not thickened." The urine was found to contain a large amount of sugar and also a considerable quantity of albumen (about one sixteenth). It had a specific gravity of 1033. Mr. Linton Brunton had often before demonstrated the presence of sugar, but we had never found albumen, nor was it found subsequently.

Mr. H— had been most of this time in a condition of very urgent illness ; often vomiting everything for days together, and not in the least expected to live. Towards the end of February his general condition was a little better than it had been, but as the foot got worse and worse we decided to amputate at all hazards. This was done above the knee on February 27th, 1881. The elastic bandage and strap were used, and Lister's precautions fully carried out. When adapting the flaps the femoral artery was observed to project between them like a small bone, and I was obliged to forcibly bend it back. It had been cut in the flaps much longer than the bone, and I feared that we should have trouble with it during the healing of the stump. None such, however, occurred ; both the bone and it proved to be sufficiently covered, and the healing was immediate and without suppuration. It is now about three years since this amputation was performed, and the patient has remained well, wears an artificial limb, and gets about almost as well as formerly. His diabetes has for some time disappeared. He has lived more carefully, but has been much troubled by ulcerations on the other foot.

These cases do not include quite the whole of my experience respecting these operations. In a case under one of my colleagues in which, about six months ago, I had pleasure in giving my vote in favour of amputation, the result was, I believe, most satisfactory, and my friends have informed me of several others.

The facts adduced, I think, justify the belief that

amputation through the lower third of the thigh is not a very dangerous operation even in advanced years and with most extensive calcareous degeneration of the arteries. I have never in a single instance had any trouble with secondary hæmorrhage nor encountered any difficulty in securing the vessels at the time of operation. I have always tied with stout catgut or carbolised silk, and have taken up the tissues surrounding the artery freely. The smaller vessels have been twisted, but I have never trusted to this for the main trunk. If it be admitted that the operation is not in itself dangerous, then I think there can be no hesitation in believing that it offers a most acceptable alternative to the miseries of death from slowly advancing gangrene.

NARRATIVE
OF A
CASE OF TRUE LEPROSY,
IN WHICH
COMPLETE RECOVERY HAS TAKEN
PLACE.

BY
JONATHAN HUTCHINSON,
SENIOR SURGEON TO THE LONDON HOSPITAL AND TO THE BLACKFRIARS
HOSPITAL FOR SKIN DISEASES; CONSULTING SURGEON TO THE
ROYAL LONDON OPHTHALMIC HOSPITAL.

Read February 11th, 1879

*[From Vol. LXII of the 'Medico-Chirurgical Transactions,' published
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1879.

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(Received December 10th, 1878—Read February 11th, 1879.)

THE subject of the appended narrative has been under my observation at different times during the past twenty-seven years. When I first saw her she was suffering from leprosy in a severe and well-characterised form. It was the mixed kind, partly tubercular and partly anæsthetic. She has now regained good health, and although in the state of her right hand, in which ulnar paralysis exists, in the dusky tint of her skin, and in its loss of sensibility in various parts, we have permanent consequences of the disease, yet there have not been for many years any indications of tendency to relapse. All tubercles have long since disappeared, and both as regards skin and nerves, we may consider that reparation has taken place as far as it was possible. As an instance of proved recovery from a disease which is usually steadily aggressive, and especially because I am able to produce the

patient for inspection, I think the case not unworthy of the attention of the Society.

It will be seen that the disease was acquired by an immigrant into a leprosy district who did not inherit any proclivity, and that the recovery took place after a permanent return to England. The patient, during her residence abroad, had been exposed to no hardships, and her case seems to me to lend some support to the opinion which I have long held that the malady is due solely to diet, and that its special cause is probably fish.

Case of recovery from severe leprosy acquired twenty-eight years ago in Jamaica.—Mrs. Harriet I—, æt. 71, was born in Portsmouth, of Jewish parents who had been both born in England. When thirty-two years of age she went to the West Indies, being in excellent health at the time. She lived at Montigua Bay, Jamaica, eleven years, and during the greater part of that time enjoyed good health. Her diet was beef, turtle in all forms (at sixpence the pound), fish and vegetables. The first was plentiful and good. She thinks she took fish almost every day, but adds, “I never took any fish that would injure me—barracouta, or anything like that.” About a year before she returned to England she began to suffer from leprosy. The first local ailment that she remembers was a swelling on the right little finger and within a month or two blotches came on her forehead. It was diagnosed on the spot as “something of the nature of leprosy,” and she was advised to return to England.

On arriving in England (æt. 44) she at once presented herself at the Hospital for Skin Diseases, where, under Mr. Startin’s care, I often saw her. We took great interest in her case as a well-marked example of true leprosy. Her face was covered with dusky tuberculous folds of thickened skin. Her arms also showed many patches, and were in parts anæsthetic. She got no benefit from our treatment, and after ten months’ attendance left us, and obtained admission into Guy’s Hospital under the care of Dr. Addison, who, she says, wished to have a wax model made.

She remained in Guy's Hospital three months, where, she believed, an attempt was made to salivate her, but without success. She next went under Mr. Hunt's care, who pushed a course of arsenic, and, she thinks, made her eyes sore. After this she was under care at Moorfields (Mr. McMurdo) for her right eye, and has continued to attend there almost ever since.

As regards the cure of her skin she states that all drug treatment had seemed to fail and she had left it off. About two years after her return from Jamaica, the skin being still much in the same condition, the return home of a wealthy relative placed good wine within her reach, and she took freely of port and champagne, under which rapid improvement ensued, and in about a year she thought herself well. She has had no return in the skin, and "has never had a day's ill health since; not to speak of."

Mrs. I— is now a very young-looking woman for her years (71), florid, and healthy. She still shows various conditions which have resulted from the leprosy. Her skin, although nowhere quite anæsthetic, excepting in the region of the right ulnar nerve, is in many parts deficient in sensation. Her right eye is inflamed and the cornea ulcerated, with deposit in its structure and much persistent conjunctival congestion. The state of her eye is that characteristic of leprosy, but there have not for many years been any aggressive changes.

As regards the frequency of recovery from conditions of advanced leprosy, it is probable that they are infrequent, and that they occur only under conditions, such as those present in this instance, of complete change of place of residence. I have never myself witnessed a similar case, nor do I know of any which have been authenticated in English practice. In former times many Norwegians suffering from leprosy emigrated into the United States, and I believe that it was thought that a certain number recovered, but I am not aware that this fact has ever been proved by the citation of individual cases. It is, indeed, not often that a patient remains sufficiently long under the observation of the same

surgeon for him to be able to speak definitely of the first and last state of such a protracted malady. I have seen during the last twenty years probably about a dozen cases of leprosy in Europeans. Two of them I know to be dead, but the others have passed from my observation. In every instance I have forbidden the use of fish as the first essential in treatment.

ADDRESS IN SURGERY,

DELIVERED AT

The Forty-ninth Annual Meeting of the British Medical Association.

Held in RYDE, August 9th, 10th, 11th, and 12th, 1881.

BY

JONATHAN HUTCHINSON, F.R.C.S.,

Professor of Surgery and Pathology in the Royal College of Surgeons; Senior
Surgeon to the London Hospital and to the Hospital for Skin-Diseases;
Consulting Surgeon to the Royal London Ophthalmic Hospital.

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—
1881.

ADDRESS IN SURGERY,

DELIVERED AT

THE FORTY-NINTH ANNUAL MEETING OF THE
BRITISH MEDICAL ASSOCIATION.

MR. PRESIDENT AND GENTLEMEN,—As we are most of us fresh from a long week's carnival of medical science, I feel sure that you will not desire that I should on the present occasion address you on any subject directly connected with surgical practice. The very successful and industrious Congress which concluded yesterday has had its numberless Section meetings, in which most of us have taken some part; and, if I do not misinterpret the general feeling, it is a longing for a little rest as regards those matters in the discussion of which we have so recently and so fully engaged. I purpose, therefore, to employ the hour which you have done me the honour to entrust to me, in bringing under your consideration certain general topics having reference to surgical ethics, surgical education, and, lastly, the best means of advancing the clinical knowledge of disease.

Five-and-twenty years ago, the surgeons of the metropolis were alarmed, I might almost say scandalised, by a proposal to open a hospital for the treatment of stone and diseases of the bladder. A memorial was got up, and signed first by the ever respected name of Brodie, and then by almost the whole of those then connected with the general hospitals. This memorial severely condemned the multiplication of special institutions, and with particular vigour denounced the one in contemplation. Amongst the names at its foot, mine may be found. I am not ashamed of having signed it then, but I unhesitatingly say that I would sign no such memorial now. Nor can I believe that many of those who then did so would now differ from my present conclusions. Not that the facts then stated have been materially changed, but others have been added. Year by year, we have seen more clearly that, in large communities, special hospitals will develop, and that it is beyond the power of the profession to prevent them. But we have seen more than this: we have seen that they are clearly a gain to the public; not an unmixed gain—for what gain is unmixed?—but still a gain. Even at the time when the memorial to which I have referred was written, it was a matter of necessity to

admit that diseases of the eye constituted an exception, and some thought that orthopædic institutions should also be permitted. Since then, I suppose that those for diseases of women, for diseases of children, and perhaps for diseases of the skin, have justified themselves.

The final triumph of ovariectomy did not result from any new discovery. There had been many pioneers. The instruments and the modes of practice which had been devised by others were those which were still employed; but these, in the hands of able surgeons, who gave their whole time and energy to this one subject, and who had, above all, the power of excluding from their special institutions those sources of evil from which a general hospital can never be wholly free—attained for this operation its present proud position. Let us note carefully the two elements of success. It may be that Listerian or some equivalent precautions may in the future make the wards of a general hospital as safe a place in which to open the peritoneal cavity as is a private home or a special hospital; but, even if this be done, they can never supply the familiarity with detail which comes only from constant practice. We must never forget that our profession exists, not for the benefit of ourselves, still less of any special class amongst us, but for our patients; and that its institutions must be so managed, or so modified, that they shall best serve their permanent interests. Nor can I think that there is any real difficulty in making the two coincide. Let the profession set its face, not against specialisms as a whole, but rather against those institutions which are conducted in a narrow spirit. Let it insist upon open elections of officers, free admission of students and practitioners; above all, let it encourage the formation of special departments in our general hospitals, since it is clearly here that they can best be made useful in the education of the student.

These general principles of conduct being granted, I would not oppose the beginning of any speciality, however detailed. Already the comprehensive department of eye-diseases is submitting to some process of natural subdivision, and it will not be long before we shall have in our large cities those who devote themselves chiefly to operations on the eye, those who attend specially to its diseases properly so-called, and those who rectify its congenital or acquired defects by means of optical aids. It by no means follows that because a man is a good operator for cataract, he will be equally familiar with the details of astigmatism. It is impossible to question the fact that the great discoveries in ophthalmology have been made by specialists, and sometimes by those who had devoted their attention chiefly to special branches of that department. Let it be understood that I am not arguing in favour of the promotion of subdivisions of surgical labour, but rather to the effect that, when they come naturally, we should not oppose them. Let us have charity for the various motives by which different men are urged to different courses; let us hold high our standard, inscribed "for the good of all", and allow to natural energy a full and free development. Let us not waste our time in opposing, but seek rather to employ and use.

I rank amongst the gains from the detailed cultivation of special branches of medical pursuit, that it has a definite tendency to destroy specialisms as such, and add their conquered territory to the general possessions. Whilst it does this to a large extent, it also at the same time creates new departments before unthought of, into which science pushes its way, and gives its help towards the mitigation of the many disabilities of man.

Witness what has been done in Ophthalmology; how discoveries one after the other have been made, which have been at once added to the knowledge of the general physician. Note how the ophthalmoscope has taken its place with the stethoscope as an indispensable aid to the practitioner in the diagnosis of disease. The knowledge of diseases of

the eye has indeed rapidly attained in the profession at large, and not alone amongst specialists, a very high degree of perfection. I do not hesitate to assert that there are few departments of practice so well and widely appreciated, and few which more definitely attract the attention of students. Nor do the gains to the public from this increase of general knowledge represent the whole gain; for the study of eye-disease must be claimed as especially useful as a training in the art of precise observation. No student masters it who does not become in so doing much better fitted for other fields of clinical research. This study is indeed in relation to other branches of surgery almost what mathematics is in general education. We have but to reflect on such facts as these, and next try to recall what the ignorance of eye-disease was less than a century ago, and what the position held by the oculist and the spectacle-vendor, and we shall be able to estimate a part of the debt which we owe to specialists.

There was a time when Diseases of the Skin were regarded by the higher class of surgeons with feelings almost allied to contempt. They were repulsive alike in the portrait, and in the person of the patient. They required no operations; and a knowledge of the use of arsenic, and of the constituents of a few ointments and lotions, was held to be all that was needed for their treatment. Then followed a period during the early cultivation of the speciality, when to outsiders it seemed as if it offered only an arena for endless wrangles as to schemes of classification, and for ingenuity in the devising of new names, and affixing to them a countless variety of unclassical adjectives. I am not quite sure that this era has even yet wholly passed away. It may be that there yet lingers a feeling of prejudice to this speciality amongst some who should know better. With the majority, however, and especially amongst our rising pathologists, a wholly different conviction is rapidly superseding such sentiments. We are learning to care little about names, and to seek knowledge as to the nature of things; and those who do this, in earnest, rapidly find out that, of all the departments of pathological and clinical research, there is none which offers such rich and varied attractions as does Dermatology. The simple facts that a morbid process in the integument is, from its beginning to its ending, exposed to view, that the aid of the lens may be brought to bear upon it while yet *in situ*, and that the histologist is in very many instances not obliged to wait the death of his patient before he is allowed to gratify his thirst for knowledge of ultimate details, largely justify my assertion. But it is not upon them solely that I rely, when I assert that diseases of the skin ought to be regarded as fundamental in professional education. Many illustrations which they afford of the changes in vascular supply and its results; of the influence of the several parts of the nervous system in the production of disease; of the laws of inheritance; of the numberless varieties in the inflammatory process in connection with diathesis, idiosyncrasy, and special forms of poisoning; are varied and instructive, far beyond those offered by any other department. Above all, we find in dermatology the most remarkable and conclusive proofs of the direct connection between morbid causes and their effects. I cannot doubt that, possessing these advantages, skin-diseases must in the future be accepted as not only of the utmost interest for themselves, but as invaluable to the physician and pathologist in the elucidation of the phenomena of diseased action, as met with in regions and organs less open to inspection. I by no means claim for this insight into the educational value of a knowledge of skin-diseases that it is wholly of modern growth. It was recognised long ago by the Paris physicians; and the work done by Anthony Todd Thompson, by Jenner, Gull, and above all by Addison, attest to its partial recognition amongst ourselves. But its growth is only recent, and never had Dermatology such

an army of workers who are not specialists as it at present claims amongst the younger physicians and surgeons, not only in London, but in all parts of the world. In their hands, it is certain before long to assume that foremost place which I claim for it as its natural position.

Let us remember, respecting a large majority of the maladies which it has been the fashion, in some sort, to stigmatise as "skin-diseases", that nothing is more certain than they belong mainly to other departments of pathology. Herpes is a form of neuritis which happens to display some of its symptoms on the skin. Morphea is an affection of the vaso-motor nerve, leading to changes in bones, muscles, and joints, as well as to those which, from occurring externally, first attracted attention, and still almost monopolise it. In the study of ringworm, we may engage, if we will, in the most interesting investigations as to the laws of life in minute vegetable organisms. Leprosy offers us a dietetic problem of at least equal interest with those which concern gout and rickets. In psoriasis and its allies, we study a heritable peculiarity of health or of skin which shows its effects through a whole life, and is influenced for better or worse in ways that are curiously instructive. Who can doubt the power of drugs who has seen arsenic cause herpes or cure pemphigus? The polymorphism of syphilis is a fact to claim the permanent wonder and interest of all who are instructed in pathological speculation.

We listened, with delight, last week, to the eloquent words in which, in his opening address, Sir James Paget enforced the duty of charity amongst ourselves. It is in this spirit, I think, that we should meet the various questions which open out in connection with the medical education of women; and that, also, as to consultation with those who, whilst educated amongst us, have openly professed their adoption of peculiar doctrines. I fear that I am here venturing upon ice that is very thin indeed; and I must proceed either with great boldness or extreme caution. I shall prefer the latter. Let me say, then, that the profession of medicine always has offered, probably always will offer, peculiar attractions to those who, with weak principles, and still weaker consciences, desire to make profit by trading on the credulity of their patients. The thing is so exceedingly easy to do. Our real knowledge of disease in many of its departments is very vague, and our knowledge of therapeutics still less certain. There is room on all sides for differences of opinion, and scope for the introduction of new theories and the employment of high sounding epithets. The fatal facilities thus afforded to the charlatan have naturally made the well principled professors of physic very vigilant in guarding our ranks against the introduction of quackery. We wish to be honest, and we wish to associate with none but those who are so. There lived, now more than a century ago, a talented and learned enthusiast, who thought—sincerely, I have little doubt—that he saw his way to an immense reform in the use of drugs. That reform was needed, we all admit. He noticed a few common facts, such as the marvellous subdivision of which odorous substances are susceptible; the change of effect, or even reversal, which accrues from change in the dose of a drug; and that, respecting some few medicines, it was true that they seemed to produce in large doses just what they tended to repress in small ones. I am not here to apologise for Hahnemann. His facts were few, his reasoning illogical, his ignorance of the natural progress of disease and its tendencies to spontaneous recovery, such as would be utterly disgraceful in the present day. Whatever foundation we may grant for some of his theories, he certainly pushed them to the wildest lengths.

If, however, we may find theme for marvel in the presumptuous self-sufficiency of this would-be reformer, I do not think that we need seek

far for the explanation of the success of his teaching. It inculcated faith in drugs, but it changed their form, and gave us cleanly globules and tasteless fluids instead of the bolus, pill, and potion. It supplied a theory of cure, as well as its means; and to the intelligent, but, at the same time, not specially trained, its theory sounded at least as good as those of orthodox physic. The love of novelty conspired with a cheerful faith in the possibility of progress, and with delight in escape from the disagreeableness of the old methods, to draw converts to the new creed. Those converts were not the ignorant, nor were they the poor. No wonder that some from our own ranks should have thought they saw their interest in adopting the new method, and equally little that most of those who observed their conduct held the motives of the man who put "homœopath" on his door to be low and self-seeking. In nineteen cases out of twenty, probably the verdict was right; but when the fiat went forth that a homœopath must be either a fool or a knave, I doubt whether the modesty of nature was not somewhat overstepped. There are fools and fools, but we are guilty alike of unkindness and unfairness if we widen that disrespectful epithet over much, and apply it too freely. There is such a thing as a combination of weak power for the estimation of facts, with enthusiastic optimism as regards possible progress, which, whilst it in no degree establishes a claim to wisdom, yet scarcely brings its possessor into the category of fools. Amongst the laity, of those who became homœopaths, most were of this class, and some, probably, of those who seceded from our own ranks.

I fear that it may be thought that I am travelling very far from the proper subject of an address on surgery. I also much fear that I may be misunderstood. What, it will be asked, has homœopathy to do with surgery; and why introduce the question of consultations with homœopaths by such a lengthy prelude? Now, it is precisely because homœopathy has nothing to do with surgery that it becomes of importance to us to settle the question in dispute. The circumstances of Lord Beaconsfield's illness are fresh in the memory of us all. We gave honour to Sir William Jenner for his stern and manly refusal to have anything to do with what he thought quackery; but we also sympathised with Dr. Quain in his perception, that the occasion had arrived for the sacrifice of sentiment and the performance of a disagreeable duty. What particularly struck me in the transaction, and what constitutes my chief reason for mentioning it now, was the reason alleged by Sir William Jenner why he could not meet Dr. Kidd. It was not that he felt compelled on principle to decline all intercourse with the heterodox, but that the patient could not possibly be a gainer by a conference between those who held such different opinions respecting the principles of therapeutics. It is clear that, had it been the aid of a surgeon that was needed, no such reason would have been valid. Homœopaths have not as yet succeeded in developing any new system of surgery. The knife and the catheter are the same to them that they are to us, and are used on the same indications. I never myself wittingly consulted with a homœopath; but I believe that I have, without knowing it at the time, several times met them, and I never yet encountered the slightest difference of opinion. The surgeon, then, cannot possibly feel that he is in any way serving the interest of the patient when he refuses to meet his weak-minded doctor. On the contrary, it may easily be the fact, that he knows that it would be the greatest possible kindness if he would go without an hour's delay. To Boycott a quack on principle is one thing, to attend to the interests of the quack's patient may be another. Hence the duties of surgeons in this matter, and especially of those engaged in consultation practice, have always been very difficult. The obvious incongruity which exists in the case of a physician is not present to the surgeon; his temptations are both more frequent and

stronger, and his sources of inward strength are fewer. He refuses neither for his own good, nor the patient's good, but in obedience simply to professional rule. With a few exceptions, this rule has been, I believe, honourably upheld by the consulting surgeons of England. There have, however, been some exceptions, and there have been difficulties and annoyances without number. Now we cannot possibly, in our profession, have one rule for the peer and another for the tradesman. I avow my deliberate conviction that Dr. Quain, and those whose counsel he sought, interpreted the obligations of our profession correctly. We enjoy a law-established monopoly in the art of healing, and we must be very careful how we stint or refuse our services when they are demanded. If, in consultation, it be found that the opinion of the consultant is not that of the consulter, and, if the latter be not willing to waive his own, then the proper course of conduct is clear. But, such inability to act in concert surely should not be assumed on light grounds, or hearsay evidence. Unless the previous knowledge be very special, it should be established at the bedside in each individual case. I am speaking of formal consultations only, not of social intercourse. We know well how to accord and refuse professional honour. If a man be guilty of non-professional conduct, we can blackball him at a society, and avoid his company in social life; but we enter upon a course of conduct which needs a wholly different justification if we refuse to meet and confer with him, when the life of a third person is concerned. Here I confess that it seems to me that the claims of the public should stand first, and that if a man's name is on the *Medical Register* we ought to meet him, so long as the consultations result in that which we deem most for the patient's advantage. Whenever they do not, our duty is clear, and we should readily know how to perform it. Many advantages, I think, result, if we were to leave with the licensing bodies the responsibility of decision, as to who are to be admitted to the privileges of formal consultation, and who excluded. To do so would save at once much loss of time and of temper, and avoid frequently recurring complexities. It would encourage honesty and openness of conduct, and remove temptation to the secret perpetration of that which is known to be against the professional rule. But, above all, I would urge that it seems to be almost a matter of justice to the third party, our patients, who have surely a right to assume that, when a duly qualified man is employed, they can obtain, in consultation with him, if wished, the aid of any other, who is engaged in the public practice of his profession. That we run the risk of fostering homœopathy by according to its disciples the courtesy of consultation, I do not for a moment believe. It has hitherto been fostered by opposition. Let us have more confidence in the vital energy of truth, and let us venture to let the wheat and the tares grow together till the harvest. We believe that its principal theory is absurd, and much of its practice ridiculous; but, at the same time, we are prepared to admit that gleams of a fruitful suggestion may be occasionally discerned in its discussions, and we can surely afford to leave it as a whole to itself, and let it develop to its natural end.*

I will pass to a less distasteful topic, and proceed to make some suggestions as to the possible improvement in our methods of clinical teaching. It must be noted, in the first place, as a great defect in our English system that it makes no provision for retaining the services of

* These paragraphs concerning consultation with homœopaths did not form part of the address, as given at Ryde. They had been written out for delivery; but on the evening before, I learnt that the same subject had been far more ably dealt with by Dr. Bristowe in his Address on Medicine. The explanation of our having selected the same subject will be easily seen. Dr. Bristowe dealt with the whole subject, whilst I have spoken only as to the inexpediency of continuing to refuse formal consultations. I have reinstated these paragraphs, in consequence of a general request that they should appear when the address was printed.

good clinical teachers. It trains them, and then casts them adrift. The early period at which our surgeons and physicians retire from hospital work is a matter of amazement to our foreign *confrères*, and few can doubt that the evil is on the increase. Either by the bribe of private practice or the gentle compulsion of a retirement rule, we induce our best teachers to desist from work in public just at the time when their services are of most value to the student. The names of Paget, Jenner, Erichsen, Gull, and Bowman will occur to us all in illustration of what I assert. Not only do our customs make the early retirement of successful men a matter of necessity, but they often cramp their usefulness to the student during the latter part of the period of their tenure of office, and also hinder their services to science in other directions. Success in our profession means absorption of the whole time and whole mental energy in attention to private practice. No individual surgeon can help himself; he is in the meshes of the social net, and escape is hopeless. It is a change of custom which must effect the reform, and surely such a change is urgently required. It might come, I think, very suitably in some such form as this. Let there be constituted a rank of pure consultants, whose fees shall be so modified as to enable them to make the same incomes that they now do with a third of the personal labour. Let election to such grade be by their respective colleges. Let us no longer leave the time and the energies of our foremost minds at the mercy of any wealthy man who, however trivial may be his ailment, determines to have "the best advice", just as he would seek the most expensive jeweller or buy the most costly wine. Such men are too valuable for that. By the plan which I suggest, leisure would be left to them for study and for teaching, and they would be retained in their proper sphere of public work. Consultants of the class I suggest, having restricted their spheres of observation of disease in private, would find it necessary, in order alike to sustain reputation and keep abreast with progress, to retain their public appointments, and to continue to teach. The gain would be a great one to medical education, and also, I think, to the public interest. The evil to which I refer is an increasing one, and it exists to a far greater extent in London than in any other capital.

A large part of the education of students may very suitably be done by young professors. Age adds but little to the ability to deal well with the facts of anatomy and physiology, and it may even detract from it. When, however, we come to the knowledge of disease at the bedside, and the correct estimation of its various symptoms, then experience must tell. Without it there can never be that trust on the part of the student or that free power of illustration from facts which have been personally observed, on the part of the teacher, which are essential to success.

Yet it is precisely this clinical teaching which is of most importance to the student, and in which, if I mistake not, our modern system is most at disadvantage. Apprenticeships have been abandoned, and few that remember their inconveniences can wish to revert to them. Yet they had their uses, and those not trivial ones. They unquestionably often secured to the student a clinical and practical training which is now but too often missed. Permit me to make for your consideration a suggestion of a plan by which possibly most of their advantages might be retained, and others which they did not always possess secured. Let us endeavour to widen the basis of medical teaching, and to enlist, as responsible partakers in this all-important work, a large section of the profession. This might be done if our examining boards were to recognise as private teachers all fellows of their respective colleges, all possessors of diplomas of the higher class, all medical officers of hospitals and dispensaries, indeed, all who could bring proof of the possession of special opportunities or qualifications for clinical teaching. Let it be required of every student that he should

bring, in addition to those now required, certificates from two, three, or more of these registered teachers that he had been—in a real and *bonâ fide* manner, under their personal supervision—instructed in practical matters, and that he was in their opinion qualified for a diploma. Make such certificates necessary, but let them not modify in the least the curriculum of the school or the test-examination. To prevent these certificates from being signed in a perfunctory manner, let it be the rule to inform those who signed them of the results obtained by their *protégés*, and if in the course of a five years' period the proportion of rejections should be more than an average, let the name of the teacher concerned be for the next five years removed from the list of those privileged to sign. This plan would, I think, work well in many ways. It would add to the value of the higher diplomas, and increase the number of those who seek them. It would encourage in those registered as private teachers the endeavour to keep well instructed in the knowledge of the day, whilst its advantages to the student—by bringing him into closer personal contact with those interested in his success—are obvious.

How various are the qualifications which are to be desired in the medical practitioner. He should be a gentleman of good manners and address; skilled alike in the principles of biology and in the knowledge of character; able to visualise at short notice the details of human anatomy; and he should carry in his memory, ready for prompt use, the best recipes for a thousand varying forms of ill-health. If we measure his responsibilities by the possibilities of his usefulness and the risks of his failure, they are very great indeed. It may be true that the greater part of his duties are routine and easy of performance, but he may, at a moment's notice, be called upon to deal with the unusual and difficult. Regarded from this point of view, the amount of knowledge required of him is enormous—greater probably than that necessary in any other avocation. It is, further, steadily on the increase; every fresh discovery brings with it something fresh for the general surgeon to master and retain in memory. Hence many of the increasing difficulties in surgical education; hence unquestionably the increasing ratio of plucks to passes at our examining boards. It is not that examiners are more strict, students less able, or teachers less zealous; but simply that the thing to be taught has grown in bulk, and become year by year more and more difficult of attainment within the allotted period. There is nothing whatever to discourage us in the fact, much, indeed, to tend in the opposite direction. But we must boldly meet the changed and ever changing circumstances. An extension of the period of study, a well considered limitation of its subjects, and lastly, a careful development of its methods, are the three measures which severally suggest themselves. The last is, of course, approved by all, and is too obviously desirable to need comment; but concerning each of the others there is room for much debate. As to the extension of the compulsory period of study, such proposals may, I think, be dismissed with the remark that the practice of liberal rejection of candidates imperfectly qualified really amounts to the same thing, and attains its end with more justice to the diligent and able. In the future, it may perhaps come to be considered a great credit to pass the first time, and no disgrace to be referred. Careful men, appreciating the necessities of the case, will probably voluntarily lengthen their period of study. Were the period compulsorily fixed at five or six years instead of four, the careless would still, as now, idle till near the end of it. I cannot but think, therefore, that the practice of early examination, with its necessary result of many rejections, works on the whole better than would any which should make an indiscriminating demand for longer time.

It is impossible not to regard without the utmost jealousy any proposal that the subjects of professional study should be reduced. So far from its being desirable to strike out botany and comparative anatomy, we might prefer to see added, if possible, a good knowledge, not only of the anatomy and functions, but also of the diseases of both plants and animals. It is from a broad education in these directions that we may hope for future advance. Having said this, however, we must hasten to admit that a large majority of the profession are to be trained, not so much as biologists, nor even as pathologists, but as practitioners. In our surgical education there is much that is valuable, very much indeed that is of the greatest possible interest, concerning which it still cannot be said that it is essential. It is certainly the duty of both teacher and examiner to draw a strong and clear distinction between essential and non-essential acquirements. Howsoever the latter may fare, the public has a right to demand at our hands that the former shall be in as complete possession as possible. It is no comfort to the glaucoma patient, who has been treated by lotions and leeches until he is blind, to know that his surgeon is a good anatomist; nor will the most excellent knowledge of histology avail to save a practitioner from something in its nature not unlike manslaughter, who believes that he ought to wait for tympanites and stercoraceous vomiting as the chief symptoms of strangulated hernia. It is common sense and practical knowledge of common things that we mainly want. I well recollect an anecdote, which was told to me when a boy, respecting a smart young farm-labourer in my father's employ. This young fellow had incurred the wrath of a half-witted young woman in the village, who, in revenge, said of him: "He can whistle fairly, and he can sing pretty well, but he can't plough straight". This home-thrust so rankled in his breast that his accomplishments became annoyances to him, and he finally left the neighbourhood, unable to bear up under the frequent reminders as to what daft Meg had said. The distinction between the essential and the ornamental was here so strongly emphasised that, although the latter was not in the least depreciated, it stood as less than nothing in comparison with the former. So surely it ought to be with us. Respecting the essential, examiners might perhaps do well to leave nothing to chance, but, regardless of time, to make sure, so far as is possible, that the candidate possesses a really sound knowledge of them. It by no means follows that a candidate who knows what to do in traumatic gangrene is equally up to the mark in reference to purulent ophthalmia; nor does a good knowledge of the latter imply ability to treat a case of prostatic retention with success. Yet these are all equally essential, and they stand pretty much in the same relation to the duties of a surgeon as does straight ploughing to a farm-labourer.

We may reasonably hope that improved methods of instruction, and the application of common sense to our plans of teaching, will do away with the need for any material curtailment of the scope of study. The means provided for the education of students may be classed under three heads: Teachers, Books, and Museums. Of the two former I do not propose to say much, but the last is an attractive topic which I cannot pass. The change which followed on the introduction of printing, in reference to the value of oral instruction, has often been the subject of comment. At the present time this change is complete, and no discoverer or propounder of new doctrines would ever think of bringing his observations before the public in any other way than by the aid of the printing press. He does not expect his hearers to come and listen to his professor's lecture, but he embodies his opinions in a book, and then sends them broadcast over the world. If, in the first instance, he reads a paper or gives a lecture, it is that it may be printed afterwards. The professor of the present day is, for the most part, an exalted develop-

ment of the tutor, and his duty is almost as much to ascertain that his pupils do really learn from books as to teach them from his chair. Few, indeed, are there who can attract hearers from outside their allotted classes. Nearly all have themselves published books, and their success, in nine cases out of ten, depends far more on their willingness to adapt themselves to the existing state of things as regards students' requirements—to advise, supervise, and question—than upon their abilities in the lines of original research. For the latter, the vocation is elsewhere. I am speaking, of course, of the professors and teachers in our colleges and schools of medicine. They have become the expositors of books, not of their own original and unwritten opinions. It is, I think, not improbable that another development is at hand, which will yet further diminish the importance of our chairs, and of oral instruction in general. I allude to the creation of Students' Museums. The museum, hitherto, has existed chiefly as an appendage to the chair. Our examining boards have required that teachers of anatomy, physiology, and pathology, should possess or create a museum of specimens from which to select illustrations for lectures. In many instances, these collections have not even been made accessible to students, and in none, until quite recently, has encouragement been given to the student to regard the museum as a place in which he ought to work. In one of our large European capitals, possessing a flourishing medical school, there was, I believe, until very recently, and I am not sure that things are materially changed now, no museum which a student could enter without the most troublesome formalities. In several other continental cities possessing medical schools, the creation of a pathological collection, worthy the name of museum, is a matter of exceedingly recent date. I claim it as a distinguishing feature of our own country, one, however, which I willingly share with our Scandinavian relations, that our museums have for long been numerous and good, and I further assert that it is amongst the most valuable proofs of life in the scientific spirit which we can show that they are constantly growing and improving. In no country, however, have we as yet seen the full development of museums as means of medical education; and, if I am not much mistaken, these institutions are destined in the future to assume an importance of which we have as yet scarcely dreamed. Well managed, it will be found that the museum may be made to combine the advantages of dissecting-room, ward, lecture-theatre and book in one, and that it can supply permanently, at all times, and to all comers, opportunities, which are for the most part accessible elsewhere only at special times and to a privileged few. I do not mean that dissected preparations or models can ever supersede or rival work done *propria manu* with forceps and scalpel, but they may be made to assist it, to prepare for it, and to supplement it with most excellent results. Hitherto our museums, whether medical or in connection with general knowledge, have been far too miscellaneous. Huge crowded collections of material, some of it of the greatest value, and some of it of very little, have jostled together in a more or less orderly kind of confusion, through which only the well instructed can find their way. They have been, by the majority, visited rather as places of wonderment, or perhaps of bewilderment, than for systematic instruction. Now museums ought to be as legible as books; and, when they are made so, they will be eagerly read. The very most that can be said of our very best, as yet, is that they have approached somewhat to the character of cyclopædias, from which fragmentary information upon all sorts of subjects may be obtained by those who know how to search for it. For students' purposes, something of a different kind is required—something much less voluminous, at once more concise and

more consecutive: in fact, to pursue the comparison, more of the nature of a *handbook*. A students' museum should contain those things which a student wants, and those only. These should be well arranged, with plenty of space; and well labelled, not merely with a name, but a description. There should be nothing to distract attention, and everything to favour study. Anatomy should be illustrated by dissected preparations, casts, drawings, and diagrams; and these should be kept in juxtaposition; and from anatomy to pathological change the steps should be direct and clear. Side by side with the normal joint should be the diseased joint; with the specimens illustrating the precise position of the epiphyses, those showing their detachment by violence. No knowledge should be taken for granted in the learner, and as far as possible everything should be demonstrated. I must not go further into detail. Let me conclude what I have to say on this topic by the remark that we ought to have museums for educational purposes, distinct and wholly separate from those which are designed as magazines of facts deposited for the use of original investigators. The two objects are different; and not unfrequently that which is essential to the completeness of the one is useless and cumbersome to the other.

In speaking of students, hitherto, I have been thinking of those who have not passed their examinations; but it is the boast of our profession that it possesses countless students of another grade, who have no longer the fear of the examiner before them, but who recognise the fact that even a lifetime is too short to acquire a fair familiarity with the facts of pathology, and who, however long their lives may be, will remain in the position of pupils. For these also museums ought to be provided; and here, again, I make bold to assert that nothing in the least adequate to the wants of the case has as yet been attempted. Our grandsons, if not our sons, will smile at the *dilettante* manner in which we have been content to hunt for the truth in matters of clinical research. We so often treat disease—at any rate, in its less common forms—as if it were sent merely that we should write papers about it, and discuss its nature in more or less detail, and with more or less seriousness, according to the humour of the hour. I do not speak now of the more practical section of the profession—men engaged in the daily and hourly discharge of routine duties, in the conscientious endeavour to apply well-known rules to the treatment of disease, and only exceptionally concerned in the pursuit of new knowledge. I speak rather of the more ambitious amongst us—and, thank Heaven, they are now very numerous—men who attend societies, compile statistics, collect specimens, and write papers. Of these I assert—may I be permitted to claim for myself a humble position amongst them, and say of *us*?—that we are far too ready to yield to the temptation of thinking that disease was made for the physician, and not the physician for disease. We investigate it in the same spirit that an amateur geologist brings to his problems—as a thing which may agreeably exercise our ingenuity and train our minds, upon which we may perhaps base our reputations, and out of which there may perhaps come some good for mankind. We claim to lay aside our work when we are tired of it, and to vote its too urgent pursuit a bore, forgetting that such investigations are to us a matter of the most urgent professional duty, and to our clients one of life and death.

That I may not seem to loose an unaimed shaft, I will take an example, and it shall be one in which almost the whole of the English profession to some extent is concerned. The museum of the Royal College of Surgeons, the Hunterian Collection with its fifty years' additions, is without a rival in the world. It is a noble museum, and has been nobly cared for by a succession of curators who have made their names

famous in science by work done within its walls. It may seem a bold thing to charge against an institution so foremost, that its arrangements are inadequate to the wants of the present day. Amongst the wants of the medical profession at the present time, and amongst those wants which alone a national museum is fitted to supply, is the fullest possible information respecting the symptoms of disease. It is not enough that we illustrate its final results, that we keep in bottles or otherwise the documents which demonstrate its ultimate conditions: we need a pathology of the living as well as of the dead; and everything that human contrivance can do to elucidate this should be attempted. Here we must mark a huge hiatus in the arrangements of our College. There is little or no endeavour to illustrate the effects of either disease or injury in the living, and an exceedingly meagre attempt to show what has been done in reference to instruments for surgical treatment. The modeller's art, of which such beautiful examples from Paris have recently been shown to us in the Congress Museum, is at our College "unknown and like esteemed". I must correct myself; for there is, in a topmost gallery, a small collection of models, the gift of our present President. These, however, are only a little series selected from the magnificent array of similar objects to be seen in the St. Louis Museum at Paris. It is imperfect and ill-shown, and the College makes no effort to add to it. A museum adapted to the wants of the practitioner should supplement the hospital, whenever possible, in the display of the outward characters of disease. Nothing in the whole range of diseases of the eye and skin or other external parts, nothing that a speculum can show, or a modeller delineate, should escape it; the rare and the common should alike be there, and the practitioner should be able to resort to its galleries when in doubt as to diagnosis, or desiring to recapitulate his knowledge, with confidence that he will there see all that can be shown. Nothing less than completeness should be the aim, and from all sources copies should be procured where the original cannot be had—the photographer, the modeller, and the artist being employed without regard to expense.

If a surgeon were now to go to our museum and ask to see models which would help him in the diagnosis of the different forms of chancre, or in the recognition of such sores on unusual parts of the body, I fear that he would be disappointed. He might find an unequalled collection of pre-historic skulls, and the skeleton of a splendid whale, but little or nothing in reference to the practical object of the diagnosis of surgical disease. Yet, in proof that it is possible to give such aid, I again appeal to the St. Louis collection.

Let it not be thought that I speak in disparagement of anthropology or comparative anatomy. It is a proud boast of our profession that its members have been foremost in these pursuits, and long may it be sustained. In their liberal cultivation, we follow the example of the founder of our museum, and keep up its most cherished traditions. But surgery has widened much since Hunter's time, and its special cultivation now asserts claims which did not then exist. These claims are, I cannot but think, primary in such an institution, which, whatever it leaves undone, should first attend to the duty of giving all that it can possibly give to the elucidation of human disease and the means of its relief. I cannot, however, believe that a liberal development in the new direction would necessitate any curtailment in the old. Let but a proper appeal be made to the profession, the public, and the Government, and means would surely be forthcoming which would enable our National Museum of Surgery so to develop itself, that no surgeon should ever spend a day in London without a visit to its collection, and none should pass through its rooms without obtaining information which would be of the utmost value to his patients.

There is yet another kind of museum which is, I think, a desideratum, and which I have no doubt the future will possess: I refer to a Museum-Hospital, in which living persons, the subjects of chronic and incurable diseases of an unusual kind should be collected and encouraged to remain, with, of course, every attention to their comforts, for long periods; every facility being offered for their inspection by all members of the profession. From such hospitals I would wholly exclude common cases, such as are useful, nay essential, in the training of the student, and would collect only those likely to be instructive to advanced practitioners. Each case should be carefully studied by competent authorities, and described in accessible catalogues. A visit to such an institution would be invaluable to the man engaged in busy general practice, and its growing records would become rich mines of information to the clinical investigator. Rare maladies are not to be regarded as mere objects of scientific curiosity, but should be utilised to the utmost, and made, if practicable, familiar to all; for in them often lies the key to the interpretation of other pathological phenomena which are common enough. Let us cease from dilettanteism, and try to economise our resources. When we do so, surely we shall find that there is a better way of dealing with examples of Myxœdema, of Addison's disease, of Charcot's joints, and Morphaea, than by relegating them to the wards of an union asylum, where they will be seen by none. I mention these merely as examples; there are many others concerning which it is equally true that, if opportunities were afforded for their collection together, under conditions of facility for research and inspection, great help would be given both to medical education and to the progress of clinical knowledge.*

I should be very sorry if what I have said, perhaps too plainly, as to what appear to me to be defects in our existing arrangements for the promotion of surgical knowledge, should have left the impression that I am in any degree a dissatisfied complainer. I hope that I yield to none in thankful appreciation of what has been accomplished in the past. But surely the fulness of the harvest which we have been permitted to reap, should prompt us to increased diligence in putting in seed for the next. We have every reason to feel encouraged and hopeful, but let us not allow the sentiment of confidence to lull us into sloth.

The chemist and the empirical seeker after new drugs may, I suppose, share the pleasure which must come from the knowledge of how iodide of potassium has made curable a whole phalanx of maladies before hopeless, and not the less full of misery because often accompanied by the bitterness of self-reproach. The operating surgeon may remember the triumphs of ovariectomy, which has restored in health hundreds of mothers to their families. If we could bring together in one place those who, thanks to the ingenuity and industry of Von Græfe, have been by iridectomy saved from blindness through glaucoma, and are now enjoying the blessing of sight, they would crowd this large hall, and leave no standing room. The abstruse optical researches of Young, Helmholtz, and Donders have borne fruit in the fact that thou-

* An experimental attempt in this direction was made by the Committee in charge of the Temporary Museum organised for the recent Congress. It was, I believe, considered successful; at any rate, although conducted under great disadvantages as regards want of room, it was largely visited. Every morning at a certain hour a selected number of living patients, illustrating for the most part very unusual maladies, were brought together for examination, brief explanations being given by those who brought them. In this way opportunity was afforded for the simultaneous inspection of groups of each of the following maladies:—true leprosy; severe gout, and mixed gout and rheumatism; myxœdema; osteitis deformans; spondylitis deformans; lupus erythematosus; primary muscular atrophy; morphaea; syphilitic teeth; xanthelasma; and many others.

sands all over the world, whose sight was comparatively useless, now enjoy it in almost full perfection. The purely practical man may rejoice in remembering how much Sayre's jacket and Martin's bandage have done, and are daily doing, for the mitigation of suffering and the cure of diseases which render life a burden. The application of the germ theory to the treatment of wounds has, I doubt not, had for one of its results, amongst many others, that at the present moment there live, scattered in very distant places, many thousands of able-bodied men, the fathers of families, now earning their children's bread, who but for it would long ago have been in their graves. It is true that we as yet see no hope of a cure for cancer, but the pathological doctrine, which is rapidly gaining ground, that many forms are local, and that the pre-cancerous stage should be vigilantly recognised and vigorously treated, is already saving many from becoming its victims.

A few weeks ago, visiting a renowned cathedral, I found inscribed on its floor at long distances apart, three remarkable words, *Credo, Spero, Amo*. I am not ignorant of the special meaning which in such association was meant to attach to these words, nor would I now for one moment attempt to employ them in a different sense, if I thought it would give pain to the tenderest conscience in this room. But indeed they are words of the widest bearing, and refer to feelings and attainments which lie at the very basis of all human character. We become what we are, we effect what we do, in virtue of what we love, what we believe, and what we hope. I have ventured to censure as a weakness to which those who work in pursuit of a more intimate knowledge of disease are very liable, a spirit of dilettanteism, a willingness to be contented with half results. For that weakness, the cure rests in the *Amo*. He who intensely loves will sympathise with the miseries which afflict his fellow-men, and will be ever zealous for their relief. Nor will he fail to use every opportunity of becoming familiar with the reality of those miseries, and thus warm his sympathies and increase his love. Next comes the *Credo*. Do we heartily believe in respect to the advancement of the happiness of man, in the primary value of the discovery of scientific truth? From the *Credo* to the *Spero*, in this instance the step is very easy, for they are almost phases of the same sentiment.

I have just enumerated, very briefly, a very few of the countless encouragements to hope which those conversant with the history of our profession may easily find in the records of the last half-century. Can any doubt that they are but an earnest of far greater triumphs to come? The work that is before us spreads out in a sort of threefold division. We have to apply in the best practicable manner our present knowledge for the benefit of those around us. We have to do our best to increase that knowledge, and, thirdly, we have to find the best means for transferring it to the new generation which will soon succeed to our duties. Medical practice, the advancement of the knowledge of the nature of disease, and the training of our sons—such are our three great spheres of duty. Some of us work in one, some in another, most of us to some extent in all. Let us seek to love the final object at which we aim, to believe in the means which we are employing, and to hope confidently of their results.

AN ADDRESS

ON

THE USES OF KNOWLEDGE

*Delivered at the Sessional Opening of the Medical Department of the
Yorkshire College, Leeds.*

By JONATHAN HUTCHINSON, F.R.S., F.R.C.S.,
Emeritus Professor of Surgery to the London Hospital College.

LADIES AND GENTLEMEN,—The part which the managers of your Yorkshire College have assigned to me in to-day's proceedings is one which I feel to be a high honour. A Yorkshireman myself, apprenticed and educated in my native county, I may assure you that it was with feelings of both pride and pleasure that I received your invitation to preside on the present occasion. The only drawback to the gratification which I felt in accepting that invitation was the knowledge that I must give an Address, and that my audience would be one for which specially professional topics would not be suitable. I will not pretend that I felt that I had nothing which I should like to say on the many and important topics which are appropriate to an educational celebration such as this. It was rather a feeling of great doubt whether I could say what I wished to utter in a manner which should be at the same time interesting to you, and not liable to be misunderstood.

In this dilemma I appealed to a much valued adviser, who is herself a native of Leeds, and who was also very desirous that I should accept the honour you had offered me. "If I say so-and-so," I asked, "shall I cause misapprehension?" "You may say what you will to a Leeds audience," was practically her reply. "If you are right they will appreciate you, and if wrong they will easily put you right." It is in the assurance thus obtained, that I purpose to address you this afternoon, and to bring before you in simplicity, without, perhaps, much, even of arrangement, and without any attempt whatever at oratorical display, certain thoughts and suggestions as regards the future of education and the increase of knowledge, on which my mind has often dwelt with pleasure and profit to itself. It may be best, perhaps, to begin at the beginning, and to ask, Why do we value knowledge? Why do we need our Colleges? Why do we meet year after year to distribute rewards for diligence and success, and to cheer each other on at the commencement of new sessions of educational work?

So far as this world is concerned, we can find, perhaps, no better symbol of human life than that given us by an evergreen tree. The leaves are the generations of men; the root, stem, and branches, the social institutions by which each successive generation is borne up into higher life. We live in the most literal manner upon the past, and are what we are in virtue of its former life. If, during any one year, the production of leaves had failed, if they had been found unequal to their task, the death of the tree, in part or in whole, must

have followed, and no further outbursting of its leaves would have been possible. Upon the vital vigour of each generation has depended, in the past, the growth of the tree. They were the digesters of its sap ; they prepared its wood. It is, of course, a mistake to suppose that evergreen trees do not shed their leaves ; they simply escape to a large extent the influence of season, and retain their leaves longer than others ; but, as anyone who has ever stood under a fir will know well, their leaves are constantly submitting to death when old, and are replaced by new ones. An evergreen, therefore, rather than one which becomes bare every winter, must be our symbol, and none perhaps better than the Norway pine. Anyone musing upon the magnificence of this splendid tree may usefully remind himself of the beneficence of the law of individual death. It is thanks to that law that its growth has been attained, and its greenness made lasting. Each successive generation of leaves has taken up the work of its forerunners, and, "eager to do and die," has in turn submitted to the same fate. The result has been increasing growth and perpetual youth. There has been permanence of life amidst unceasing change. So is it in the human family.

It needs but a few moments' thought to reconcile us to the beneficent institution of individual death, and to make us see that it helps the progress of mankind, and interferes not in the least with our real immortality. The life which seems to be lost, is simply transferred to others. In many other details the symbolism on which I have ventured might be developed, but I have said enough for the present purpose. In one remarkable feature the human tree differs from the pine. Not only do its successive generations of men inherit from the parent stock their life-vigour and its tendencies, but they are capable of receiving, by direct communication with their predecessors, a vast wealth of impulse and power, which has not as yet assumed a form in which its acquisition by inheritance is possible. This communication must be voluntary, and to it we give the name of education. Were the habit of death abolished, and did personal life last indefinitely, the need for education would almost cease, for although our patriarch pupils would probably be learning still, it would be rather in classes for mutual instruction than in school or lecture-room. As it is, the most important duty of each generation, next to that of the hereditary transmission of sound organisation, is the transference of its own acquired knowledge to the minds of its successors. Laborious and endless as this transference-task is, we can easily see that some advantages attend the scheme. Especially in reference to the advance of knowledge, its changes of form, and the additions which it is constantly receiving, can we perceive that it is a gain that young and fresh minds should be offered to its influence. The prejudices, the narrow formularies, and the erroneous conceptions of the past, are thus much more easily left aside. No process of cleaning the battered soot-begrimed leaves of last season could compete with nature's plan, which covers the tree with new and untarnished foliage. If, however, we accept thankfully the ordinance of death, we must not neglect for a moment to remember the responsible duties which it imposes upon us. Each generation has to select, as best it may, from the enormous stock of floating knowledge, those portions which it deems most likely to be useful to those who will follow it. With each generation this task becomes more and more difficult, for the sum increases with wonderful rapidity. The botanist, the geologist, the statesman, the surgeon, one and all have to consider what is to be taught to their successors, how far and in what respects the instruction received by themselves must be modified or extended for the use of the next generation. Not a single department of human knowledge, turn where we will, but is growing with a rapidity which, alike to the teacher and learner, may well seem appalling. A child asked as

to which reign she would like best to have been born in, chose that of William Rufus, on the plea that then she should have had but one date to remember. There are times when the teacher may almost sympathise with her sentiment, and wish that his vocation had fallen in earlier times, when the mass of knowledge to be taught and learned was somewhat less than it now is. We may, indeed, admit without cavil, that one of the most valuable faculties of the teacher of the present day is the power of bold determination as to what may safely be left unlearned.

The capacity of the human mind, varying much in individuals, is yet limited in all. It is not so much defective power of comprehension as of retention. We can easily understand a hundred things for one that we can clearly and permanently remember. Most humiliating are, I suspect, the defects of memory to us all. I have no wish to assert that all that has been forgotten is wholly lost. On the contrary, we may safely believe that much that has faded away, past all possibility of recall, has yet helped very efficiently in the general development of our minds, and been in this way a lasting gain. This consolation, extend it as we may, is, however, only a very partial solace for the loss of our facts.

This law of limitation in reference to memory is, I think, not sufficiently taken account of in education. It is most surely the fact that the packing of our minds is like the stowage of a cargo. We cannot put in more than a certain quantity. Dexterity in arrangement will go for something, but it has its limits, and when these are reached, it is impossible to put in fresh articles without displacing those already there. The busy man, with his mind hurried from one topic to another, and still more those whose minds are exposed to grief, sorrow, or anxiety, will realise this, and may sometimes amaze their less occupied brethren by their capacity for forgetting. The power of remembering riddles is a fairly good test of the power of memory when unaided by intellectual associations. I have seldom found any of my friends of middle age who could remember six. Children, whose minds are unstored, and who are not constantly accumulating facts, can often retain very many; but for myself, I may confess that if I try to acquire a seventh, it always displaces one of the original six. This physical limitation in the number of our brain pigeon-holes is a fact which we do well to remember. Do what we will to improve and develop our faculties—and we can do very much—yet sooner or later a limit is reached, and we are then compelled, however reluctantly, to choose what we will keep, and what we must surrender to the waves of oblivion. Nor is it less the part of wisdom on some occasions to determine beforehand resolutely to forego the acquisition of new knowledge when we know that it will displace that which we already have and which we value more.

A few words must be said as to the relative value of different kinds of knowledge. To most of us, some special departments are matters of duty, and beyond the range of choice. We must all keep our minds clear of everything which is likely to interfere with the memory of those facts which are needed in our professional lives. For knowledge of this kind, we are responsible to the community. The surgeon who allows the facts of astronomy to push those of pathology out of his head, acts unjustly towards those who employ him. How well the public recognise this danger, we all know. It is difficult, indeed, to persuade our clients that some brains can find room for proficiency in other subjects as well as in medicine. Although absurd occasionally in its developments, we cannot doubt that this prejudice is well founded. The knowledge of a man's own profession being then allowed its paramount position, we come next to the selection of other subjects. What are the uses of knowledge? and is it better to aim at a superficial acquaintance with many subjects, or a detailed familiarity

with one or two? As regards the greater part of the community, I unhesitatingly record my vote in favour of an acquaintance with things in general. Let those who have leisure, or industry, or special aptitude, cultivate to the utmost special sciences, and restrict themselves as closely as they like. It is from them that new observations and discoveries are to be hoped, and for such we will pay any price. They are, however, and always must be, a minority. For the chance of a discovery, be it in astronomy or entomology, we can well afford to pardon almost any amount of ignorance in political economy or geology. Where discoveries are not to be hoped, however, then ignorance in any department is much to be regretted; for it limits at once a man's enjoyment of life and his usefulness to others. The prospectus of your Yorkshire College is neither a sheet nor a pamphlet, but a bound volume of very respectable size; and, when we use the honourable term "a well informed man," we mean one who knows at any rate a little about most of the subjects which are merely catalogued in it.

Let me press the question as to the uses of knowledge. Our estimate of those uses, if I mistake not, has advanced greatly of late. Not only do we now value knowledge more, much more, than we did, but we assign to it a quite different kind of worth. The time was when, apart from what was needed in a man's avocation, his attainments were looked upon as a sort of ornament, as that of which he might be proud, but as by no means essential to his usefulness as a citizen. It was believed that a man might be wise without possessing knowledge, and honest and loving without being even wise. We have, however, of late taken broader views, and recognised the fact that we have been using different terms for things essentially the same. That the noblest faculty of the mind, the noblest emotion, if you prefer that term, is Love, is a fact to which all religion, all poetry, the general verdict of mankind, bear witness. His is the best endowed organisation who can love the most widely, and the most intensely. Why is the capability of affection thus a test of the strength and vigour of the character? I answer, because it implies sympathy; it implies sense of brotherhood; it implies perception of beauty; and all these, in turn, imply the faculty of insight; in other words, the possession of knowledge. Love and sympathy are not abstract qualities, springing up causelessly in our hearts; they are the results of hereditarily transmitted faculties, by which we take cognisance of beauty, be it moral or be it physical. If we could not perceive the beauty, we should never feel the love. A little reflection will, I think, convince any one that this is so, however numerous and prominent at first sight may be the apparent exceptions. We shall be misled if we allow ourselves to be embarrassed by isolated and ill-estimated facts. We must look broadly at their whole range; and, doing so, I am sure we shall be obliged to admit that the power of loving wisely, well, and strongly, is always proportionate to the intellectual development of the being. Obviously, if we go downwards in the scale of organisation, we shall come to a point at which the want of development is such that we cannot conceive of the possession of personal affection.

It is the same with what we call wisdom, a faculty or possession which has often been set in a sort of superior contrast with knowledge, as of distinct and nobler birth. The more we think, the more sure shall we feel that there is no such thing as wisdom apart from the matured results of hereditarily transmitted knowledge. No ignorant person born of ignorant predecessors can possibly be wise. In the divine order of human events, there are no means of intensifying the faculty of love, or increasing the endowment of wisdom, other than by obtaining better knowledge. It is ignorance which is the foe of both. Our great poet never penned a more comprehensive line than when he declared, "There is no darkness but ignorance."

It is clear, I trust, that, in thus speaking of knowledge, I am not thinking of school-books. I mean the real, the living thing, the sight of Nature, the perception of the real, the revelation of the true; and my estimate of its transcendent value rests on the assumption that the real, could we get at it, is beautiful, and of a character and quality to claim and secure our love. He who cannot grant this, lacks the fundamental sentiment of reverence, and can obtain, so far as I can see, no foothold for his faith. "Let good men feel the force of Nature, and see things as they are."

Having thus, so far as my feeble powers of expression permit, exalted knowledge; having claimed for it no lower position than this, that it is the seed—possibly in itself not beautiful, but still the seed—of that plant of which wisdom and love are the flower and the fruit, I need say but little as to the worth of the office of the teacher. If faithful, single-hearted, and zealous in his task, he may rest assured that, whether working alone in a parish-school, or as a part of a great collegiate organisation such as that which I now address, he is engaged in that which is simply the noblest vocation of man. "The teachers," said Richter, "shall shine for ever, like stars in the firmament of Heaven." Next to the ambition to discover something new, which, after all, is only a higher form of teaching, the desire to be the means of spreading knowledge is the noblest aspiration of the human mind. It is so because it shows faith in light, because it is inclusive of all other aims. "Let there be more light" will ever remain the morning and evening prayer of every benevolent man.

Permit me to try to illustrate what I have asserted in general terms as to the fundamental relationship between knowledge and the development of what we term the moral instincts, or the affections. It is time that I should try to come to close quarters with my topic.

You will understand that I am speaking now not of the kinds of knowledge which are obviously useful in the affairs of daily life, but of those which men and women may get or not, as they like, and which are usually considered matters of personal pleasure or social adornment. Surely, if we think of it, we ought to rank some knowledge of astronomy, geology, zoology, botany, natural philosophy, human history, and the like, as essential to the formation of character. If the truths which they teach be once ingrained in the mind, so that they become the bases of all its processes, the person concerned has become capable of meeting the changes of life with patience. He has learnt that best of all lessons, the doctrine of continuity. It is impossible for him to despair of the world's progress, or to think lightly of the value of individual work. He has seen in how wonderful a manner great things are made of little things, and how in the past nothing has really perished or been lost, but how, on the contrary, all force has been conserved, and how, through periods of time too great for the faintest attempt at realisation, the progress of the world has been ever onward towards better things. He has seen with his mind's eye the great trinity of Nature, the three persons of which are, the past, the present, and the ever brooding life. It is quite impossible for anyone to rise from such a contemplation without a firmer faith in the Fatherhood of the Eternal, a deeper and a warmer sense of brotherhood with all that lives.

In order that such result should be obtained, what is needful is that the knowledge should be real. It should be a matter of heart-conviction. So long as the distances, the sizes, and the numbers of the star-worlds are simply entertained by the mind as matters of speculation, so long will they be barren of further results. So also if the age of this planet, the succession of life upon its crust, and the history in particular of the human race, are esteemed as matters of mere opinion, they will profit nothing. It is only when we begin

to believe in earnest that we reap the reward of knowledge. The sceptic and the half-believer must not hope for the joys of faith. The mellowing influence of creed on character cannot come to him who doubts.

It will require several generations for us to realise the full influence of recent revelations in science. The doctrines must have been taught familiarly in childhood, their influence must have been transmitted hereditarily, before we shall be able to see what it really is. It would shake my belief in the relation between cause and effect if I could doubt that the influence of modern knowledge in reference to the history of the world and of man, and the relation of man to animals, could be other than ennobling. We shall witness from such causes an increased sense of dignity in life. Mankind will become more sincere, more earnest, more sympathetic, more hopeful, and our gain in all these respects will be the result chiefly of increased confidence in terrestrial life.

To those who know something of what schools were half a century ago, there are few subjects more suited to "breed perpetual benedictions" than the state in which they are now. The improvement has been immense, but it is still far from complete. We emancipate ourselves only slowly from the traditions of the past. The value which Latin and Greek possessed in the days when English literature scarcely existed, still lingers as a tradition of great force amongst us. We have, it is true, risen above the belief that the one sole object of school-teaching was to ensure, by any method, and often by those which were little better than brutal, a boy's knowledge of those tongues; but we still submit our minds helplessly to a superstition that they are essential to a liberal education. By some strange accident, for which we can never be too thankful, Hebrew has been left aside; and it has been tacitly assumed that, as regards the most important book of all, it is enough to read it in translation. With regard to Homer and Virgil, it is different, and few making any pretensions to education would be willing to admit that they had ever read them in other than their original tongues. The study of these grand languages, and of many others, must ever remain, for teachers and for specialists in literature, of the utmost importance; but I am concerned to here express emphatically my conviction that, as matters of general knowledge, it is a great waste of time and memory to teach them. Perhaps there are few subjects which make greater demands upon the memory and the retentive power than the details of a strange language. The remark of Heine was as sound as it was witty, when he asserted that it was well for the Romans that they were born knowing Latin, for if they had had to learn it, they would never have had energy left to conquer the world. There are three main arguments in use by those who defend the retention of these languages in their present place in the educational course: first, that the study tends in an especial manner to develop the mind; next, that they are the key to an invaluable literature; and thirdly, that they are essential to a sound comprehension of our own language, especially of the terms in use in science. To these it may, I think, be justly answered, that the study of a language is for the most part a mere matter of rote memory, and encumbers rather than strengthens the mind; next, that there now exist abundant facilities for becoming familiar with the literature and history of Greece and Rome, without the toilsome acquisition of their modes of speech. May it not be added that, when Homer, Virgil, Tacitus, and Cæsar, shall cease to be school-books, and when it shall be held not discredit to be acquainted with them in translations only, a sound knowledge of them will probably become a far more general possession than it is at present?

The third objection is one which it is far more difficult, in the present state of our scientific literature, to meet. There is not the

least doubt that the student who knows not Greek and Latin is at an immense disadvantage in respect to his reading in botany, zoology, or any of the sciences. Our text-books teem with words which are derived from these languages, and of these the meanings are most difficult of memory to anyone not previously possessing a good vocabulary in those languages. For this purpose, however, let it be observed, a good vocabulary is sufficient. It is not necessary to study grammatical construction in detail. Even against this necessity, real as it is for the present, let me enter a most earnest remonstrance. Surely the preference of the professors of science for terms taken from the dead languages is again the mere survival of a bad custom, and one from which it is our duty to free ourselves as soon as possible. Never will the knowledge of the sublime facts of science become as wide as it ought amongst us until we effect a reform in this matter. A young person unskilled in languages, but fully able to enjoy the study of botany or geology, opens an elementary book on either of these subjects. It may be one professedly of first lines, such as Professor Oliver's excellent little introduction; but, be it large or small, he will find in every page such words as *gamopetalous*, *monocleous*, *syncarpous*, *pleistocene*, *miocene*, and the like. If he looks them up in the glossary, he still, at each recurrence, finds it an effort to recall their meaning. Now, every one of these terms might just as conveniently, and in many instances far more elegantly, be expressed in English. The number of English-reading students is now quite sufficient to encourage some enterprising publisher in the production of books from which all needlessly learned terms shall be rigidly excluded. It would be a little effort at first to get accustomed to their English equivalents. Some little ingenuity would doubtless be necessary in devising them; but, if once the thing were done, the gain would be immense. Let it be understood that we do not want works made easy in the sense of being childishly explanatory; of these we have enough. All that is required is, that no word shall occur the full meaning of which would not be clear to a person knowing only his mother-tongue. Not only are these pedantic words a source of hindrance, in that to many they are incomprehensible, and to all a burden on the memory, but they bring with them other definite evils. The mind unconsciously associates its own meaning with a word which it is compelled to use, but does not understand, and thus often is not only left ignorant, but actually led astray. Further, I am sure that it is true that a high sounding name often leads us to believe that our knowledge is far more definite than it really is. Science would be greatly simplified if all its exponents were compelled to express their meaning in modest English. I will not, for want of time, venture to illustrate what I mean in asserting that the reform asked for would not be very difficult.

We should be sorry to miss many of the beautiful names which have come into use; and regarding many of these, it does not matter in the least whether or not we attach any meaning to them. They are simply names. The objection which I urge applies chiefly to descriptive names, and especially to appended adjectives.

In thus venturing to suggest that, as regards the progress of English students in general, it would be a great gain if the dead languages were forthwith honourably buried, I am quite aware that I shall do great violence to the feelings and tastes of almost all well educated persons. We naturally prize highly the possessions which have cost us dear; and great also, I fully admit, are the intrinsic claims of classical lore. My protest is this, that, at the present stage of the world's progress, such lore is very far indeed from being our most important acquisition, and that the present custom of exacting it is a great bar to real mental development. May I venture one step further, and say that the learning of modern languages is also, in the present day, far too much

insisted on? We have inherited a sort of infatuation in this matter, and accept the dictum without a question that the first thing for a child to be taught is the language of some other nation; if a boy, it must be Latin and Greek; if a girl, French. The outcome of it is usually, in the one case, a rooted antipathy to the classics, and in the other, an unwilling acquaintance with the adventures of Telemachus. To many of us, a knowledge of the modern languages is a matter almost of necessity in daily life; but to by far the larger number of those to whom they are taught they are useless. The time and labour consumed upon them would usually be far more wisely devoted to other subjects. A young lady will be contentedly ignorant of the very first principles of geology, may not know the title of one of Mrs. Browning's poems, may think Wordsworth dull and Robert Browning incomprehensible, and may yet pride herself upon being able to speak French. Of those who have spent months or years in the acquisition of German, how few ever make any compensatory use of it. Goethe is not attractive to many; of Schiller we have translations as good as the original; and *Soll und Haben*, read as a matter of duty, is the summit of the attainments of most.

I do not believe that the acquisition of a new language is at all easier in youth than in adult age; and respecting modern languages in general, would suggest that it would be wise to delay learning them until a definite need for them has been realised. That a large number will do wisely to acquire them, I do not doubt; but that it is necessary to make them an essential part of the education of all, I am, I repeat, concerned to deny. If once we could exempt ourselves from the routine of fashion, rid our minds of the prejudices born of long-established custom, we should see that foreign languages are means only, and not in any sense ends; that, for the majority, they are almost useless, and that it is quite possible that, over and above the loss of time and brain-fatigue involved in their acquisition, they may prove only a memory-burden in the end. Far better that a young man should know well the general course of French history, and be familiar with the lives of its heroes, its men of science, and its saints; that he should have followed carefully the struggles of the Huguenots, and watched the long-gathering causes which led to the Revolution, than that he should know ever so perfectly the irregularities of every French verb. The one kind of knowledge is living, is in itself fruitful in incentive to sympathy, to action, and to vigour in social life; the other is, for the most part, dead and abortive. It will be a great gain to English education when a man is left full liberty of selection in this matter for himself and his children, and when he will feel no more shame in admitting that he knows neither German nor French than he does now in confessing ignorance of Spanish or of the ancestral tongue of us Yorkshiremen, the Norse. Why, if we must learn French, should we not also make a *sine quâ non* of Italian? Surely, neither Molière, Racine, nor Fénelon, can compare in value with Dante; yet, without disparaging the genius and sublimity of the great Florentine, I will venture to express a personal opinion that a man in the nineteenth century may find more food for his soul, more that is fitted "to stir, to soothe, to elevate," in any fifty pages of the *Excursion*, or, for the matter of that, of the writings of John Ruskin, than he will in the whole *Inferno*.

Let me not be misunderstood. I would be far from wishing to deery any branch of learning. I heartily, though at humble distance, applaud the declaration of Bacon: "I take all knowledge to be my portion;" and I would fain, if I dared, say, with Mr. Browning's *Grammarians*:—

"Let me know all;
Prate not of most or least,
Painful or easy;
E'en to the crumbs
I'd fain eat up the feast;
Aye, nor feel queasy."

What I have wished to say is, simply that we should do our best to put a right relative value on things, and not waste time and labour in planting laurel-bushes in mistake for apples. Let me urge once more, and even at the risk of being wearisome, that we must distinguish between the knowledge that is fruit-bearing, and that which is likely to prove barren, however ornamental or fashionable it may be. Nor is it in the least wished to suggest any sudden change. The study of languages must obviously long keep its present place in the educational curriculum. As yet, they are well nigh essential to a very large number of students, certainly to all engaged in medicine.

To those who mourn over the frailties of memory, it is source of much gratification to know that year by year the means of acquiring knowledge are being developed and improved. It is objective teaching which really helps both understanding and memory.

In the development of that objective teaching which will be the great gain of the future, the formation of educational museums, and the improvement of educational examinations, are two of the most important features. When the history of museums is written, it will be observed, I believe, that the medical profession has always taken a very large share in their formation. We have always been zealous in observation and lovers of facts; and not only in pathology and zoology have physicians and surgeons delighted to accumulate the materials which afford proof and illustration, but they have been foremost to perceive the value of such collections in respect of scientific observation in general. It would be fitting, therefore, if we should give impetus to a further development in the art of making and using museums, which is much needed. We have been content long enough that museums should be used by a few, and simply gazed at by the many. It is time that we set ourselves earnestly to make them what they should be in our educational scheme. Hitherto they have been mainly collections of the elements of proof, too unwieldy in extent, and too miscellaneous in character, to be within the grasp of the general student. Even in our own profession, how very little has usually been the amount of industry bestowed by our students on the task of mastering the contents of the hospital-museum. Some attention to them has of late been made needful by the customs of examining boards; but, I believe, I may still venture the opinion that only a few of our best men have learned to resort habitually to the museum-shelves as being, next to the hospital-wards, the most profitable place for study. The majority despair of being able to gain much for themselves from the interminable rows of bottles, and are content to trust that their lecturers will bring forward in the class-room the selected ones which are of importance. What we want is museums arranged specially for the student's needs, the specimens few in number, and, by the aid of label and catalogue, telling their own tale. The museum should be by no means restricted to bones and bottles, but should avail itself liberally of the art of the photographer, the modeller, and the artist. The appearances presented during life should thus be brought into easy juxtaposition with the conditions found after death. Our museums should cease to be wholly allotted to *post mortem* pathology, and should include also the clinical element. If this department were well developed, our museums might, indeed, so far as things of which the eye can judge are concerned, become an invaluable supplement to the hospital-ward.

Leaving professional topics, and returning to speak of museums as means of general education, I have to say that it is to be hoped that the time will soon come when every town, however small, and every school, will have its educational collection of objects illustrating the history of man and the natural sciences. These collections should not be large, and they should be supplied with clear catalogues, carefully freed from all pedantry, which would enable any person of ordinary intelligence to

understand them. All objects should have detailed labels, and be helped in the best possible manner by photographs and drawings; the museum would thus become a richly illustrated book, which those who would might read. It would be a place to which teachers would take their pupils, not once a year as a holiday-outing, but frequently, and as the best method of serious study. Never until something of this sort is accomplished will museums take their proper place; and never till they do will the natural sciences become attractive to the young, and a knowledge of them be easily got, and well retained.

Our botanical and zoological gardens ought, in the same way, to be better utilised for objective teaching. What, indeed, hinders that every inclosed city-square should, in the first place, be open to all; and, in the second, should contain a miniature collection of suitable zoological and botanical objects; and, again, on a very small scale, a conservatory and a museum. In all these, the objects displayed might be, from time to time, exchanged under the direction of a central organisation, and thus a constant succession of new and interesting material would be maintained. Were I to give my own answer as to the hindrance to the realisation of such a scheme, I would say that it is simply the ignorance of those concerned as to the amount of good which might be accomplished.

I had intended to say something as to possible improvements in our methods of examination, but time scarcely permits. What we all aim at is that examinations should be made to encourage industry and the attainment of retainable knowledge; and that they should, much less than they do at present, offer incentives to mere cramming. What we all wish, also, is that they should be made more certain in their results; that they should take more accurate measure of those who submit to them; and that their verdicts should be less liable to question and criticism on the part of teachers and lookers-on. These desirable objects will be attained only, so far as I understand the matter, by making examinations more objective, and by taking more time for them. I am not sure that they might not with advantage be subdivided much more than at present. These are matters of detail upon which I cannot now enter. It is, however, impossible to leave the subject without remarking that the improvement in methods of examining is a matter of the deepest interest to all concerned in education.

I have left but little time for what, perhaps, ought to have been the chief part of my discourse, that is, for addressing remarks to those present who are now commencing the study of medicine. If, however, what has been said as to the scope and aims of study in general has been made clear, it is not needful that I should add much in reference to that of medicine in particular. I shall, however, do injustice to my own feelings if I neglect to congratulate the medical students present upon their choice of a profession. The study of medicine has an advantage over many others which are adopted as means to livelihood, in that it is itself full of scientific interest. To this it may be added, that it is a pursuit to which no knowledge comes amiss. You can scarcely secure knowledge in any direction without finding that it may be made collaterally useful in your daily pursuits. Carefully avoid taking a too narrow view of the life's work which is before you. First, unquestionably, and as a thing which admits of no excuse for its neglect, you are to acquire a matter-of-fact familiarity with the common diseases and accidents to which the human frame is liable, and their means of remedy. In these matters, you must put aside all crotchets, and seek to become, as far as possible, compendiums of empirical knowledge, and embodiments of common sense. Your first duty is to advise people for the best when they are ill, and from this duty there must be no shrinking whatever. Your vocation will not,

however, end here, for your training will have fitted you to assist in the formation of social opinions in a great variety of directions. The influence of food and clothing on health; the hurtfulness, or otherwise, of such articles as tobacco, tea, and coffee; the desirability, or otherwise, of abstinence from alcohol; the relative value of different climates to different states of health; the causes of mental disorders; these, with many others, are questions upon which it is important that trained medical observers should form and express sound opinions. I need not remind you of the invaluable services to sanitary science which have been rendered by a Leeds surgeon, who has, at the same time, maintained a very high position in the profession which he adorns. If you would speak from more than mere conjecture or caprice, if you are honestly desirous that what you say in daily conversation shall be helpful to those who hear it, you will find incentive for hard work in many directions. Acquaintance with the structure of the world in which we live, the peculiarities of soil and atmosphere, a wide knowledge of the history of man and his development in civilisation, a familiarity with plant-life and with the facts as to health and disease amongst the lower animals, will each and all in turn prove of great value. In addition to all this, you will, I trust, many of you, become, in the several districts where your future life may be cast, leaders in the cause of general scientific education. Our profession is, indeed, in this matter, little less than a sort of unendowed Church of Science, sending its pastors and teachers to reside in the most various and distant parts of the world, and become bearers of the torch of knowledge wherever they may go. Nor have I the least doubt that the usefulness of our profession in this way is destined to large increase.

There are yet other subjects upon which I might offer you my unfeigned congratulations. You have had the good luck to be born in Yorkshire, and you have wisely followed it up by becoming students in a Yorkshire college. I feel sure that you will never have cause to regret either of these important steps in life. Leeds has always held a high position in respect to her medical men. Talent, indeed, seems to be hereditary here, after a fashion which we do not witness anywhere else; and the Heys, Teales, and Chadwicks follow each other in line like princes in their own right. Nor, I may safely add, did the repute of the Leeds School of Medicine and Surgery, great as it has been in the past, ever stand higher than it now does.

I might congratulate you, also, upon good fortune in respect to the times upon which you are entering. Whether we regard the development of our own profession or the general progress of mankind, unquestionably we live in an age of most unusual interest. Great advances have been very rapidly made, and there is every indication that still better things are to follow. At the same time, it is impossible not to both see and feel that problems of no ordinary difficulty are being forced upon us for solution.

I have invited you, gentlemen, to dignify the calling of medicine which you have chosen; permit me now to invite you to dignify yourselves who have chosen it. I remember well a story which is told, I think of John Foster, that he having once, in a fit of melancholy, declared that the world was "a wild beast, untamed and untameable," his friend reminded him that, at any rate, he himself was part of it. "Yes," he rejoined, "a single hair at the end of its tail." I prefer, I may confess, and I am sure that he would have done so in a cooler moment, the comparison with which I began my address, when we likened individual men to single leaves on a mighty pine-tree. As each leaf in quietness and confidence does its duty, so may it be with us. To every student here I would say, and say very earnestly, "to thine own self be true." In hours of lassitude or moments of temptation remember this maxim, and, acting upon it, all good things will fol-

low. Do not think lightly of your own individual value in the world's work, and always think hopefully of the world's progress. The world, you may feel sure, never had a more happy day than that in which you were born. Lest you mistake my meaning, let me hasten to add that it will be happier still on the day you die ; and permit me to take leave in urging the reflection that the ratio of its gain in happiness will depend, in some degree, now and for ever, upon how you live.

ON THE RELATION
OF
CERTAIN DISEASES OF THE EYE TO GOUT.

BEING
THE BOWMAN LECTURE,
Delivered Thursday, November 13th, 1884,

By JONATHAN HUTCHINSON, F.R.S.

GENTLEMEN,—At the request of your Council I have the honour this evening to deliver the first Bowman Lecture. This lecture has been instituted with the design of gratefully commemorating the services of Sir William Bowman to science, and more particularly those which he has rendered to this Society in his capacity as its first President.

It is intended that the lecturer shall on each occasion undertake the investigation of some special subject in connection with Ophthalmology.

I do not think that any more appropriate method could have been devised by which to pay respect to one of our very foremost workers. For myself, I can only say that I feel deeply the responsibility of the yet very agreeable duty which has devolved upon me, and could heartily wish that your Council had selected some one more capable of doing it justice. Having made this personal apology, I feel, however, that I shall best honour the name of Bowman by trying to imitate the example of his life, and

proceeding at once, to the best of my ability, to deal with the subject in hand.

I take for the topic of my lecture the relations which exist between certain Diseases of the Eye and Gout. We shall have to classify several different forms of disease—for the most part, but not exclusively, inflammations of the iris—to describe their peculiarities and to examine the evidence which is forthcoming as to their real connection with gout. The inquiry is an important one, for a gout taint is of extremely common occurrence in a large proportion of the British population. The investigation is also in many of its ramifications one of great intricacy and difficulty. Certain facts we shall, I trust, be able to establish with clearness, but respecting many others we shall not be able to get further than the stage of more or less confident inference. I had at one time intended to bring before you a good deal in the form of attempted proof—that is, of statistical or tabular statements of fact. I have found, however, in the preparation of the lecture that the time at my disposal will not permit of more than a brief sketch of the whole subject, and I shall therefore, on compulsion, spare you much of the detail. It is, however, only just, both to you and to myself, to state that my arguments and assertions this evening will be based upon the careful examination of a lengthy series of recorded cases. A large part of these will be found given in detail in vols. vii and viii of the Ophthalmic Hospital Reports, and the remainder I have in type before me, although not yet published.

DEFINITIONS OF GOUT AND RHEUMATISM.

Before we proceed to inquire whether gout may be deemed to be the cause in some instances of such diseases as hæmorrhagic retinitis, recurring iritis, glaucoma, and others, it may be well to state that I wish to keep a clear distinction between gout and rheumatism. This distinction

is in practice often very difficult and sometimes quite impossible to draw, but for all purposes of accurate clinical pathology we must insist upon its existence. By gout I shall mean all states of health which are, whether directly or remotely, connected with the accumulation of lithate of soda in the blood, as the result of overfeeding or defective assimilation.

Under the laws of inheritance of morbid tendency it may easily be the fact that a person may feel the results of this condition in his parents, who may yet never himself exhibit it. We may have a form of inherited gout which is not associated in the patient with either lithiasis or lithæmia. Hence, as I shall have to show presently, some very peculiar forms of morbid action.

Thus it may perhaps be conveniently laid down that there are two fundamental conceptions of what gout is. They are not antagonistic, but rather in mutual co-existence. In practice the two methods of influence constantly modify each other.

What I may call the primary conception of gout regards it as due to a state of the blood induced in part by errors in feeding, and in part by defects in assimilation and depuration. It has regard to the food, the digestive power, the habits as regards exercise, and the integrity of the kidneys. In this form of dietetic, or humoral, gout the stage of lithæmia is essential, and the final product of deposit of urates in the tissues almost equally so. We must, however, seek a wider vision than this. As an introductory stage let us imagine the condition of a man who many years ago was subjected for a long period to the ordinary causes of primitive or humoral gout, but who for a considerable series of years has carefully avoided them. In such a person it is easy to see that although his blood may have been long kept free from excess of urates, yet, as the consequence of conditions of long duration in the past, his tissues may have received modification, and may be prone still to suffer in a peculiar manner when exposed to the ordinary exciting causes of

disease. In particular they may be liable to grow old on a peculiar type. Not only are his tissues in general modified, but those of his nervous and vascular system have probably been especially influenced. Hence the power of control over any process of inflammation, by whatever cause it may have been initiated, is peculiar and defective. If such a man sprains a joint it will not recover in the way it would have done had he never had gout, yet the joint may nevertheless be very far from assuming a typical gouty state. There may be no tendency whatever to the deposit of urates. Now let us take another step in the attempt to give reality to our conceptions of the hidden influences under which the every-day phenomena about us are produced. Let us imagine that such a person as we have supposed, one who has suffered severely, and through many years, from humoral gout, becomes a parent. His offspring will inherit his tissues, his tendencies as regards digestion, kidney excretion, skin elimination, and all the rest. These will, no doubt, give a greatly increased proclivity to common typical gout should the causes of it come into play. They may, however, do much more than this. They may render their subject liable, quite independently of exciting causes, or with but little help from them, to forms of inflammation or of degraded nutrition, which are not unequivocal gout, but which are yet the direct consequence of it in predecessors. The modification which is here inherited is, I repeat, of the tissues and not of the blood, though it is obviously very easy that the two may complicate each other. The subject of tissue modification from inherited gout may easily become the victim of humoral gout as well.

INHERITED GOUT.

It is clear from this argument that we must be prepared to see inherited gout assume very different forms from those in which we are so well accustomed to recognise the

primitive type. Permit me now to suggest that it is the clinical, or rather the social, fact that a not inconsiderable proportion of the well-to-do classes of the British population are born under the conditions which I have been supposing. The number of those who are the descendants of parents who have suffered from humoral gout is very great indeed, and, especially in our large towns, it is by no means restricted to the wealthy classes. We must be prepared, then, to encounter, not unfrequently, peculiar types of morbid action which derive their peculiarities from this association. It is the duty of the clinical investigator to carefully examine the evidence which is forthcoming as regards each before he admits its claim. Certain diseases of the eye for which this claim is with more or less confidence put forward will form part of our topics for discussion this evening. It may, perhaps, facilitate matters if I express at this stage the belief that most of the varieties of what is called "rheumatic gout" stand in this relation to true gout. The list would include lumbago, sciatica, neuralgia, crippling rheumatism, arthritis deformans, osteo-arthritis, many forms of iritis, and many cases of glaucoma. I by no means wish to suggest my belief that any one of these is, in all cases, in some degree dependent upon the inheritance of gout. Some of them are doubtless in not a few instances purely rheumatic, and this admission renders it desirable that I should say two or three words as to what our conception of rheumatism ought for the most part to be. Let me hint that from our primary idea of rheumatism I should like to exclude all causes having reference to food or assimilation, and to count only those which regard climate and weather, and especially exposure to cold and damp. The rheumatic diathesis, like that of gout, may become ingrained, and at the same time modified, by hereditary transmission, and in this connection it is easy to overlook its original causes and even to believe that it may begin spontaneously, or in consequence of other influences than the true ones. The hereditary diathesis of rheumatism

(arthritic susceptibility to weather) may easily co-exist with that of gout (arthritic susceptibility to diet), and each of them or both together may combine in endless variety with other forms of morbid tendency. It follows that we have to study many hybrid, or mongrel, types of disease in more or less close connection with them.

“HOT EYE,” IRRITABLE EYES, QUIET GOUT, &c.

There is a condition which, for want of a better name, I have for long been in the habit of recognising as Hot Eye. It is one of the many curious phenomena which attend quiet gout. I use the term quiet gout as distinct from acutely paroxysmal gout. In families liable to gout, for one person who becomes the subject of acute attacks of podagra—what is known as “unequivocal gout”—there are often half-a-dozen who are the subjects of minor symptoms which denote a similar tendency. In them the tendency never rises to sufficient height to induce a severe and characteristic paroxysm. They are nevertheless liable, after taking beer or wine injudiciously, as to quantity or quality, to experience slight pricking pains in joints, attended by lithic acid in the urine, and other unmistakable signs of the diathesis. In many, these symptoms occur in such invariable association with the causes suggested that it is not possible to doubt as to their nature. The liability varies with the weather and time of year, and it often ends, unless precautions are taken, in a sharp attack in the great toe. The terms “suppressed gout,” or better, “undeclared gout,” are often used in reference to it, but since it is simply a minor form of the disease, not in any way suppressed, and certainly not to trained observers undeclared, I prefer the term quiet. Amongst the frequent occurrences which denote its presence are repeated short attacks of congestion of the eye. Usually one only is affected, but sometimes both. The conjunctiva becomes red, and the eyeball feels hot, and pricks, as if sand were

in it. The attack may come on within half an hour of the meal which has disagreed, and it may last a few hours, or a day or two. Sometimes, owing to interference with the ciliary muscle, vision is slightly dim, and all attempts at accommodation are usually painful. Those who are liable to "hot eye" not unfrequently in the end suffer from iritis; indeed it is not unfrequently an introductory stage to that disease. On the other hand, many are liable for years to very frequent attacks of it without apparently any risk of its assuming more serious proportions. In the intervals of the attacks, the eye is usually quite well, and it is indeed its definitely paroxysmal character, its sudden development, and very complete and rapid disappearance, which especially mark its arthritic relationship.

In connection with this subject it may be noted, that not unfrequently those who suffer from unequivocal gout, experience shoots of sharp pain in the eyeballs, the nature of which they well understand. A man, William B—, æt. 51, who had suffered much from gout, and whose father and four of his father's brothers had all had it, complained much of this kind of pain, yet he had never had iritis. When he was gouty, he said he frequently had "darts of pain through the middle of each eyeball," and occasionally across the bridge of the nose.

A further practical note I must ask leave to make. It is to the effect that many cases of irritable, hyperæsthetic and easily tired eyes in young persons are in association with inherited gout. I feel sure that in some of these cases we are in the present day in danger of pushing the recommendation of spectacles to an excess. Some slight degree of hypermetropia may be detected, and it is assumed to explain the irritability of the eye. Yet glasses do no good, and in fact only increase the irritation. In such cases, very often, the real malady is inherited gout.

TRANSVERSE CALCAREOUS BANDS OF THE CORNEA.

I must admit, in the commencement of our investigation, that little or no evidence exists as to the deposit of urate of soda in any of the structures of the eye. I doubt whether it has ever been proved in any single case, although several observers have suggested its presence. I trust, however that we are long past the stage of belief which regards such deposit as essential to a gouty inflammation. The nearest approach to proof of gout deposit has been made probably in the case of what are known as the transverse calcareous bands in the cornea. Sir William Bowman, in the year 1848, was, I believe, the first to publish cases of this kind, but the earliest case had been observed by Mr. Dixon. Valuable information respecting the disease has recently been collected and studied by Mr. Nettleship. I believe I may fairly sum up the present state of our knowledge by saying that no one has yet proved that the salt deposited is that of gout. It is usually calcareous. The disease occurs in persons very likely to suffer from gout, and sometimes with a gouty history, but we are not as yet justified in holding that this earthy deposit is in direct connection with that diathesis.

ARTHRITIC IRITIS.

It will be admitted, without dispute from anyone, that there are several forms of *Iritis* which are arthritic in their origin. In other words, they occur to those who are liable to attacks of inflammation of joints, and the inflammations of the eye occur under conditions similar to those which excite the inflammations of the joint. Both are in some cases prone to occur in an acute but transitory attack. Both are very apt to recur after periods of complete immunity, whilst in other cases both may assume a

chronic, persisting, and destructive form. With such features of similarity we are justified in assuming, as indeed is done by all authors, that they are due to the same causes. The problem which I have to consider to-night is whether these causes belong to gout or to the rheumatic class ; whether they are sometimes the one and sometimes the other ; and lastly, whether they are not in some cases of a hybrid or mixed nature. Permit me briefly to state some of the peculiarities which are to be observed in different types of arthritic iritis ; and to ask respecting each what evidence we possess which would connect them with the tendency to gout. Let us take first the common form in which the iritis occurs in acute transitory paroxysms. Of this, good typical examples will be in the memory of us all. Its subjects are usually men, and often of vigorous health, and past middle life. The iritis scarcely ever affects both eyes at the same time, but occurs first to one and then to the other, sometimes keeping to the same eye during several successive attacks and then leaving it, to assail its fellow. The attacks are often acute, beginning very suddenly, and attended with great pain. When they subside they subside completely, and leave the eye without the slightest degree of irritability. The duration of the severity of an attack may vary greatly at different times in the same patient. The tendency to form adhesions is extremely great. It is very remarkable how, in most cases of this class, the disease restricts itself to the iris and shows no tendency to disorganise the globe. Thus a patient in whom, in consequence of repeated attacks, the pupils may be occluded, with the exception of mere pin-hole apertures, may yet continue to enjoy very fair sight. I by no means wish to imply that this is usually the case. The case of Dr. Curry, as recorded by himself in the ' *Medico-Chirurgical Transactions*,' is a good type-form of this disease. Dr. Curry had suffered much from true gout. I may cite, however, as another good type-illustration of this class, that of a gentleman whose friendship I had the privilege to enjoy during a long period of

my early life. He was a florid, fair-complexioned man, of vigorous habits, a large eater, and through life accustomed to drink beer. He inherited rheumatism, but there was no known history of gout. He had suffered in boyhood in consequence, as he believed, of too free bathing when hot, from rheumatic fever. When I knew him first he was about five and forty, and from that time onwards to his death, at the age of sixty, he had a variety of arthritic complaints—lumbago, sciatica, chronic arthritis of the knees and other joints, and recurrent iritis. Both his pupils were closed with the exception of very small apertures, through which, however, he still continued to see enough to follow an occupation in which everything depended on good sight. I attended this gentleman year after year, usually in the early part of February, for most severe attacks of iritis, never affecting both eyes at once. He was benefited by large doses of iodide of potassium, and the free use of blisters. At length his attacks ceased to occur, and during the ten years preceding his death I believe he had not had any. I made a post-mortem examination of his body, and found no proof of gout. His knees contained loose cartilages, and there were patches where the articular cartilage was wearing away, but there was no urate of soda.

I have the record of a considerable number of cases in which the liability to these recurrent forms of iritis occur in those who were the subjects of unequivocal gout. But there were a far larger number in whom the concomitant symptoms were of rheumatic arthritis and not of true gout. The question remains, however, whether this form of rheumatic gout, although unattended by lithate of soda deposit, be not in reality a hybrid disease possessing, in addition to an unquestionable share of rheumatism, an admixture also of gout tendency. The infrequency of the disease in women, the age, the habit of body, and the mode of life of those who are usually its subjects, seem to me to strongly support this view. So does also the extreme severity of the attacks, and their rapid and

complete subsidence when once the point is turned. When this form of iritis does occur to women it is never, I think, so acute or so paroxysmal as in men; and it almost always happens to those whose relatives have suffered from gout.

VARIOUS FORMS OF ARTHRITIC IRITIS.

Next to this form of recurrent iritis, and including perhaps some of its examples, I have to mention the iritis which occurs in connection with gonorrhœal rheumatism. All observers will admit, I think, that iritis associated with ordinary rheumatic fever is infinitely rare. So also is it in connection with that type of arthritis to which the name of crippling rheumatism may be given. In other words, iritis in association with either acute or chronic rheumatism is a thing that we scarcely ever observe. In men, however, the rheumatic affections, whether acute or chronic, which are induced by gonorrhœa are not unfrequently accompanied by iritis. This iritis is less distinctly paroxysmal and much more liable to persist and be destructive than is the case in the form which I have just described. My explanation of this is easy. The reason why gonorrhœal rheumatism so often causes iritis is because it occurs usually to the subjects of inherited gout.

A third form of arthritic iritis is one which affects women more frequently than men, which not unfrequently attacks both eyes at once, which is apt to spread to the ciliary region and choroid; to persist and to prove destructive. In a very considerable proportion of the women who suffer from this form there is a history of gout in former generations.

Another group of arthritic iritis might be constituted of cases in which the malady happens to young patients, and proves but slightly, if at all, liable to recur. These single attacks of iritis without history of gonorrhœa or syphilis, and occurring in young persons, usually, I think,

affect the male sex and almost always there is the history of gout in relatives.

I must just mention, in order to complete my classification, although it is but of little clinical importance, the group of cases in which iritis occurs in association with glycosuria. In these, according to my experience, the patient is almost always the subject of gout also.

To sum up then, I would say, that although in many individual cases of arthritic iritis there may be no proof of liability to gout in either the patient or his relatives, yet the tenor of the evidence in general is in favour of the conclusion that when iritis occurs there is in reality some gout complication. The more purely and definitely is the case one of rheumatism the less the probability that iritis will happen. When iritis occurs, the complications are almost invariably those which suggest what we call rheumatic gout, rather than rheumatism pure and simple; thus the smaller joints are often affected and *nodi digitorum* present, whilst *sciatica*, *lumbago*, and *neuralgia* are frequent complications. I have elsewhere tried to prove that gonorrhœal rheumatism occurs, in nine cases out of ten, to those who inherit a gouty constitution and that it is chiefly this inheritance that gives the proclivity to it. I cannot stop now to recapitulate the evidence on which this belief rests, but if it be trustworthy it offers an explanation of the fact that iritis is so frequently met with in association with this type of rheumatism.

In certain cases of iritis with arthritic associations a very peculiar condition is observed. I allude to the filling of the anterior chamber with a soft, gelatinous, jelly-like mass. This material, which produces an appearance most alarming to the uninitiated, concealing the pupil and suggesting entire opacity of the cornea, is susceptible of very rapid absorption, and may, in the course of a very few hours, clear right away. I believe that in some cases of syphilitic iritis this peculiar form of effusion has been noticed, but if I were to speak from my own experience I should cite it as a symptom very strongly indicative

of gout. I would make the same remark, though with less confidence, respecting the rare cases of iritis complicated with hæmorrhage.

STATISTICS AS REGARDS ARTHRITIC IRITIS.

My report on arthritic iritis, published in the 'Ophthalmic Journal' of 1872-74, contains the narrative of 104 cases which are suitable for our present purpose; for various reasons I omit a few on account of doubts as to their nature or imperfections as to the details. The report comprises cases of all varieties in which either rheumatism or gout, or the two together, were supposed to have been the causes of inflammation of the eye. They were collected from my note-books, both from hospital and private practice, up to the date mentioned. It is right to state that at that time my opinions as to the influence of true gout were less definite than they are at present, and that my knowledge as to the means of recognising it was also less. The notes of many of the cases had been taken some years before they were published, and thus certain inquiries which I should now carefully make were unfortunately omitted. On this account I have no doubt that we must consider that the report does not show the influence of gout so strongly as it would have done had the cases been taken with better knowledge. Out of the 104 cases I find a positive history of gout having occurred in the patient in only eighteen cases, and only in twenty-nine was there the history of positive gout in near relatives. It is to be observed, however, that these numbers take cognizance only of what has been termed "unequivocal gout," namely, of those in which an unmistakable attack of inflammation in the great toe or other single joint had unquestionably occurred. It omits altogether the much larger number in which the gouty tendency had remained undeclared, or had been manifested only in what we may

term its quiet form. Anyone who will examine for himself the narratives to which I refer, will, I feel sure, agree with me in this opinion. In a very large proportion there was, I think, good reason for believing that a taint of gout existed where the narrative mentions rheumatism only; the arthritis had constantly been of the chronic type, and had very frequently presented exceptional conditions. If we may count such maladies as sciatica, lumbago, neuralgia, nodi digitorum, gonorrhœal rheumatism, rheumatic gout when affecting the smaller joints, as being for the most part indications of gout rather than of pure rheumatism, we shall then find that facts at any rate suspicious of gout are present in nearly all the cases. It is, I think, decidedly exceptional for rheumatism pure and uncomplicated to show any tendency to attack the tissues of the eye. Iritis in conjunction with rheumatic fever of the ordinary form is almost unknown, and even in cases in which patients suffer from repeated attacks, as is sometimes seen, the eyes usually remain free. So soon, however, as changes of type occur, the disease showing a tendency to attack the smaller joints, or to affect only single joints, or to pass into the chronic form; then we get the liability on the part of the eye to suffer. On this point my own experience is quite in accordance with that previously expressed by an acute and zealous clinical observer, the late Dr. Fuller. Dr. Fuller in his first edition, speaking of rheumatic gout, stated that in 11 out of 101 cases which he had tabulated when Medical Registrar to St. George's Hospital, the eye had suffered more or less severely, and in 14 out of 193 cases which had subsequently been under his own care in the hospital. In a second edition he stated that he had so far modified or developed that opinion that he had come to believe that many, if not all, of the cases in which this complication occurred, were examples of obscure gout or else of gonorrhœal rheumatism, adding, "This at least is certain, that since my attention has been specially directed to this

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question, I have been enabled to find a "gouty or venereal (meaning, I think, gonorrhœal) taint in every case in which the eye has been inflamed in connection with presumed rheumatic gout." This is strong testimony, stronger certainly than I am prepared to give, but I doubt whether it much exceeds the truth. Among other authors who have distinctly recognised gout as a cause of diseases of the eye, and appear to have referred a considerable proportion of those diseases, when arthritic, to gout rather than to rheumatism, I may mention Dr. Jacob, Mr. Weller, Mr. Middlemore, Dr. Copland, and perhaps Mr. Wardrop. Mr. Middlemore goes so far as to think that he has seen in one or two cases the pupil occupied by calcareous concretion of a gouty nature. None of these observers have, I think, recognised any other special disease as connected with gout than recurrent iritis. Almost invariably I find sclerotitis put down as rheumatic.

INSIDIOUS AND DESTRUCTIVE IRITIS FROM INHERITED GOUT.

I must now enter upon one of the most important divisions of my lecture. It is the attempt to prove that there is a very peculiar form of destructive iritis, occurring for the most part in young persons, which stands in all cases in direct relation with the inheritance of a gouty constitution. It does not occur to those who themselves suffer from attacks of gout, but to their descendants.

It was in 1863 that I first saw, at Moorfields, a girl named Mabey. She was then about eighteen, tall, well-formed and florid. She was too florid, and the circumscribed areas of colour in her cheeks varied in tint from bright red to slightly livid, according to the coldness of the day. She was the subject at the same time of a most peculiar form of arthritis of the last joints of all her

fingers, and of double iritis. Her right eye was lost and painful and I accordingly excised it. Her left was saved only by repeated iridectomies and finally by extraction of the lens. After the last operation she continued for five years to enjoy good sight and was free from relapses. Her fingers also got well. At the age of twenty-six she became the subject of phthisis and died, I believe, at about thirty. I showed this patient at the Hunterian Society, and drew attention to the unusual features of her disease. Someone present, looking at her hands, remarked, "Surely this is gout." I took the hint, inquired into her family history and found that in all probability it was gout. Her father had suffered repeatedly from that malady in an unequivocal form, and he had tophi in his ears. I show you drawings of the state of the girl's fingers. In order to ascertain whether there were urate deposits in connection with the swellings, I cut into one of the largest and obtained only a soft jelly-like substance. When, after some years, the swelling subsided, the terminal joints were all left disorganised and the last phalanges were more or less displaced. There appeared reason to believe that a feeble state of the circulation combined in this case with the gout inheritance.

I have repeatedly mentioned Mabey's case in public and with it others which came under my notice subsequently and I must content myself on the present occasion with a short summary of the facts. In 1872 I gave a lecture at Moorfields on this disease and was able to cite four well-marked examples of it. In 1880 I gave, at the London Hospital, a second lecture, in which I adduced seven additional cases, making eleven in all. Of these eleven patients four were females and seven males, but it happened that the most typical and severe forms of disease occurred in females, a fact which my subsequent experience has fully confirmed. In all cases both eyes ultimately suffered, but only in one were both affected simultaneously. In three instances "last-joint arthritis,"

that is to say destructive inflammation of the last joints of the digits, occurred. In all the cases vitreous opacities as well as iritic adhesions were developed. In most of the cases the eye which was last attacked suffered most, and a decided tendency was shown for the disease to come to an end as the patient advanced in life. In several cases the eye was saved by repeated iridectomies, and in one or two change of climate seemed to be very beneficial.

In all the cases but one the family history of gout was clear and strong, and in the exception the patient was the son of a brewer's man, and his father had died early, so that it was very possible that his proclivities had not declared themselves. In several of the cases the patient had been reduced almost to blindness. In three one eye had been excised and in one both were quite lost. In all excepting two the disease had begun between puberty and the age of twenty-five, and it would appear to be the fact that the earlier it begins the more severe it is. Such then was the state of my facts in 1882 when I put my last lecture into print. I think you will allow that I was justified in avowing a strong belief that inherited gout was the real cause of this peculiar form of iritis.

That I may add a little more colour to my picture I will ask your permission to state briefly the facts of three cases which have come under my notice since the date referred to. They are all cases of great interest and two of them illustrate a fact which I had previously only once or twice noticed, namely, that there is a tendency to the formation of cataract.

The last case which I have seen is that of a Miss D—, æt. 30, the daughter of a surgeon in the country. She was brought to me with the left eye quite destroyed, and the right nearly so. In the left an opaque lens had undergone spontaneous absorption and the iris was everywhere adherent to the opaque capsule. In the other the lens was half opaque and the iris extensively adherent. I was told that I had myself seen this patient ten years

before and had then recognised only commencing cataract in both. The patient's father, an intelligent medical man, was astonished when I told him there had been extensive iritis, and assured me that his daughter had never had any attacks of inflammation. The patient herself said that she had never noticed more than that the eyes had sometimes been red and hot and would prick a little. We have here then a good instance of the very insidious course of the disease. In the left eye there was no perception of light, and very probably the vitreous was affected. I found on inquiry that in Miss D—'s family there had been much gout. She herself appeared to be in good health.

Mrs. O— is a young married lady who has borne three children, and during lactation in each instance suffered from a chronic form of almost painless iritis. Both pupils are almost excluded. Her father and several other near relatives have suffered from gout. She is of feeble circulation and liable to chilblains.

I have kept one of my best cases to the last. Miss L—, æt. 22, is the youngest of a family of eight, and was born after the death of a father who had suffered much from "chalk gout." Her eldest brother has had both gout and rheumatism, and there is rheumatism also on her mother's side. Miss L— began to suffer in her eyes at the age of twelve, and at first it was simply redness and irritability with "black specks," "balls," and mist. At the age of fourteen she had a severe attack in both and was several months under Mr. L—'s care "almost blind." From this she recovered, but at eighteen a surgeon in Liverpool performed an iridectomy on the left on account of exclusion of the pupil. A little later Mr. C— did an iridectomy in the right. Neither of these operations resulted in any improvement of sight. In 1880 a surgeon in Leeds removed a soft cataract. The eye was lost and Mr. C— excised it a few months later. Miss L—'s present condition is that with the one remaining eye she can just puzzle out $\frac{20}{200}$.

Could I possibly produce before you a more marked example of an insidious destructive disease, progressing in spite of the most highly skilled assistance to its melancholy end? Yet it is only a fair example of about half those included in my series.

RELAPSING CYCLITIS.

There is a peculiar form of chronic inflammation of certain parts of the eyeball which clinical observers have recognised under the name of *Relapsing Cyclitis*. It is a cyclo-kerato-iritis involving the ciliary region of the sclerotic, the adjacent part of the cornea, and the iris. Sometimes one of these structures suffers more than another, but usually all are involved. It seldom damages the pupil itself much, seldom invades the centre of the cornea, or only very late in the disease, and it does not show much tendency to involve the choroid or vitreous. It usually begins in one eye and only affects the other after a long interval, and to the last one usually suffers more severely than the other. It may begin in early life, but often does not do so till middle periods. When once it has begun it never wholly leaves its victim, but continues either to persist with slow chronicity or recurs over and over again after intervals of health. It causes scars in the ciliary part of the cornea, thinning and discolouration of the ciliary region of the sclerotic, and ends either by inducing staphyloma or by making the whole cornea dull. It may occasionally become needful to excise the eyeball on account of the persisting irritability combined with great impairment. It affects, I think, women more frequently than men. It is a very peculiar type of disease, and it would not be difficult to place side by side a group of examples of it all exactly alike. I know of no treatment short of a complete change of climate which does much to benefit it. In this respect it is much like the form of iritis which I have been striving to prove to be a

direct result of inherited gout. Yet I am by no means in a position to produce before you, respecting relapsing cyclitis, such evidence in reference to its gout origin as that which we have been discussing. I dare not, indeed, do more than suggest that it is very probable that in some cases the constitutional cause of this destructive and persisting disease may be gout. I have found a gout history in some of my cases, but in others it has been absent. The last case which I have seen has been the one in which this suspicion was the strongest.

Miss D— is the daughter of a medical friend. She is now forty, and she has suffered all her life from her eyes. She was treated when two years old by Mr. McMurdo for what was called strumous ophthalmia, and which proved very intractable. Since then she has had numberless relapses, and in both eyes the cornea at its edges has become extensively opaque. Her left eye is the worst, and has recently been so troublesome that I was inclined to recommend its excision. It is by no means a blind eye, but the cornea is so hazy that it is of little use. Now, say that we have here a case in which strumous ophthalmia has persisted through life, let us ask what is the constitutional peculiarity which has conduced to this exceptional result. The lady has been well cared for all her life, she shows no other signs of struma, nor are such present in any of her relatives. Her father is a robust man. Mark that both eyes have suffered, and that in both it is the peripheral parts of the cornea which have been chiefly involved. Now, this lady's maternal grandfather and two of her maternal uncles suffered severely from gout, her mother had an attack in her great toe, and even she herself has on one occasion had an attack of it. This strong gout history is certainly by far the most definite fact which I can obtain as explaining the peculiarities of her eye disease.

CAN GOUT CAUSE NEURITIS ?

Although from time to time strong suspicions have been expressed as to the occurrence of gouty affections of the nervous system, I believe the question has seldom been definitely put, "Does gout cause neuritis?" We know that gout can cause the tissues of a joint to inflame, and that œdematous effusion with cell proliferation, in fact the ordinary phenomena of inflammation, are its results. I shall be much disappointed also if it is not regarded as proved that it can cause the tissue of the iris to inflame in a precisely similar manner and with like results. Can it attack any of the structures of which the nervous system is composed? Can it cause inflammation of ganglia, of nerve-trunks, or of their investments, or may it indeed attack the central organs? Without venturing to suggest that any of these occurrences are common, it is yet difficult to deny the possibility or even probability in exceptional cases. We can understand the influences that are likely to localise gout; its exciting causes may come into play far less frequently in reference to the nervous system than to the joints; or even to such an apparently exposed part of the organism as the eye. Still, if we admit that, in the gouty condition, inflammation may attack the cellular tissue in any part of the body, it is difficult to suppose that the cell elements which enter into the formation of nerve-trunks, for instance, will always escape. I am glad to note in the progress of neuro-pathology of the last few years that there seems an increasing tendency to recognise the possibility that not a few nervous affections may be due not to central disease, but to primary inflammation of the connecting trunks or of the peripheral organs. We know for certain that chronic neuritis, probably beginning peripherally, is a part of leprosy. It is highly probable that it is often a part of syphilis and quite possible that it occurs also in locomotor ataxy. How else explain many of the transitory forms of ptosis, &c., which occur in the two latter diseases?

GOUTY NEURITIS OF THE OPTIC NERVE.

The question which I now wish to put definitely is this : Are there any cases of inflammation of the optic nerve, or of any of the motor nerve-trunks of the eyeballs, the direct cause of which is the existence of a gouty constitution ? The decision is of importance not only in reference to diseases of the eye, but because it would throw much light on certain other obscure affections met with in this disease. Is sciatica really a neuritis of the sciatic trunk or its sheath ? Are any of the forms of gouty neuralgia really produced by gouty neuritis ? If the optic nerve can inflame from gout why not the pneumogastric or the phrenic ? I am sorry to say that I have exceedingly little evidence to bring forward in answer to these important questions. I will mention briefly one or two facts which are suggestive, and which I hope will prove sufficiently so to induce a more careful investigation of the subject. In two or three cases I have attended young ladies, of families in which gout had been prevalent, who suffered from attacks of ptosis, with the other group of symptoms referable to paralysis of the third nerve, for which I could find no other more plausible explanation than that they were caused by gouty neuritis. In one of these the attacks were transitory and recurrent, several having occurred within a period of a few years. I must not venture to trouble you with details of these cases, but feel myself obliged to speak rather more at length concerning one in which the optic nerve itself was inflamed, there being, I think, fair reason to suspect that the cause was inherited gout. A young lady of sixteen, was brought to me from Ipswich, on account of blindness of one eye, in July, 1879. She had no other symptoms whatever, was florid and appeared to be in good health. The ophthalmoscope showed nothing but what was quite normal, and had it not been that her pupil dilated more widely when the other was covered, I might have suspected

that she was feigning. A week later the changes were conspicuous, the disc being much swollen and its margin concealed. Under treatment by mercury combined with quinine, in the course of a few weeks the attack had passed away, but the disc was left pale. Four years later I saw this patient again with similar symptoms in the other eye, and I was now told that she had several times suffered from transitory attacks which always occurred during spring. Her health remained perfect. There was a strong history of gout in the family, both her parents having suffered repeatedly. It seems to me not at all improbable that these recurrent attacks of transitory neuritis have been really of a gouty type. In saying this, I have regard to the entire absence of the more ordinary causes and concomitants of optic neuritis, to the recurrent nature of the affection, its want of symmetry, and the exceptionally strong family history, both parents and many other near relatives having suffered. I cannot call to mind any other case of recurring optic neuritis, with gout history, which I could at all fairly place side by side with this one. I have, however, several times seen optic neuritis occur in women who were of gouty family without any of its ordinary accompaniments and clear away entirely under treatment. In none of the cases of which I am thinking did any recurrence take place, and in most of them both eyes were simultaneously attacked.

OTHER FORMS OF GOUTY NEURITIS.

Before quite taking leave of this important question as to the possible occurrence of gouty neuritis, I must trouble you with yet one other case. A lady of fifty-six came under my observation in whom for several weeks the left facial nerve had been quite paralysed. I observed at once that she had had two large iridectomies done downwards and outwards, and on inquiry found that she was nearly blind. It was twenty-three years

since she had first noticed cobwebs before her sight, and fifteen since she had last been able to read. The iridec-tomies had been done six years, with what object I do not know, for although she said that she had once had rheu-matic inflammation of her eyes, there did not appear to be any adhesions. In each eye the lens was partially opaque, and in each there were extensive choroido-retinal changes with pigment deposits and waxy atrophy of the discs (a peculiar form of retinitis pigmentosa). Mrs. H— told me that she had a sister who also had “amaurosis.” I persuaded her to bring her sister, and may now state the family history which belongs to them both. One of their brothers suffers not unfrequently from attacks of unequi-vocal gout, and a sister is crippled by rheumatism, but their parents died aged, and are not known to have suffered. The elder of the sisters, aged sixty-nine, has herself had gout, and describes her great toes graphically as having been swollen and “red like tomatoes.” This sister after her first confinement, aged twenty-seven, had an attack in her eyes which lasted several years attended by great pain and intolerance of light. Her recovery from it was very gradual, but finally it was almost perfect, and I could now find no material changes. For several years she has been very deaf and she has suffered severely from neuralgia and chronic arthritis of wrists and fingers. Once she was told by Mr. Critchett that she had “gout in the eyes.” The younger sister describes ten years ago an attack of shoulder neuralgia attended by torticollis which kept her in bed six weeks and was accompanied by agonising pain, as if her arm were in the fire. She also has suffered most severely from neuralgia on many occasions.

Here then is a family so definitely gouty that a brother and sister have each had true gout and several other sisters are crippled by rheumatism ; one sister has become almost blind with neuro-retinitis, and has suffered from neuralgia, torticollis, facial paralysis and an attack which was prob-ably neuritis of the brachial plexus. Another sister is

deaf, has been all her life liable to neuralgia, had a several years' attack of pain in her eyes which was called "amaurosis," and in later life one which was named "gout in the eye." It seems highly probable that we have here an instance of gout affecting, at different periods of life in each sister, different parts of the nervous system, and attended by true neuritis of various nerve structures.

GLAUCOMA IN RELATION WITH GOUT.

I had intended to have said a good deal as to the nature of glaucoma and the evidence as to its connection with gout. The subject is, however, too important to be discussed hurriedly, and I have not time to attempt to do it justice. I must therefore content myself with the mere expression of opinion that in the production of this extraordinary disease a gouty tendency often takes a large share. More particularly would I suggest this in the exceptional cases in which glaucoma shows itself in early life, and I could, if time permitted, bring before you some important cases illustrating this point.

RETINITIS HÆMORRHAGICA AND ITS CONNECTION WITH GOUT.

There is yet another definite and well-characterised affection of the eye which claims conspicuous mention in connection with our subject.

Retinitis hæmorrhagica is very rarely indeed seen excepting in those who are themselves gouty. It is a disease of middle life or of advancing years, and in its more typical forms is never seen in the young. It seldom happens to both eyes at once. It is attended by great swelling of the disc and adjacent parts of retina and by such turgidity of the central vein that I was at one time tempted to believe that it depended primarily upon throm-

bottic phlebitis of that vessel. It is possible, however, that such is not its true pathology, and that the venous distension and stasis, perhaps in some instances thrombosis, are really secondary to the neuritis. If this be the fact and neuro-retinitis be the primary condition, then we have in retinitis hæmorrhagica an instance in proof that acquired or humoral gout may become the cause of neuritis. It is not, I think, ever seen in association with the inheritance only of a gouty constitution, but is found usually with lithæmia, in free livers who have experienced unequivocal attacks. The case which Jaeger took for his beautiful plate in illustration of the disease, was that of a tavern keeper of plethoric habit and bloated appearance. Nothing is said as to gout, but it is fair, in such a man, to assume that there was a tendency to it. In order to determine statistically, as far as practicable, the association of retinitis hæmorrhagica with gout, I have tabulated twenty-four cases—with the exception of the one of Jaeger's, all from my own note-books. In twelve of these, exactly one half, the patient had suffered definite attacks of gout, and in five others there was strong presumptive evidence of a gouty constitution. In seven there was no proof of gout. This last group comprises two in which diabetes was present; one in which albuminuria existed, and two in which the retinitis was not very well characterised as of the hæmorrhagic group. If I had kept more closely to the type of cases illustrated by Jaeger's portrait, I should have been able to make yet stronger statements as to the almost invariable association of the disease with gout. It may be of interest to state a few other facts deduced from my table. Thirteen of the patients were men and eleven women. The youngest was forty-five. In seventeen cases only one eye was affected, and in seven both. In some cases there were hæmorrhages only, with little, if any, evidence of neuro-retinitis.

SUMMARY.

I must now bring my lecture to a close, and in doing so may, I think, venture to assume that it has been shown to be probable that there are many different forms of inflammation of the eye, or of parts of it, which are in connection with gout. Some of these are very peculiar and specialised types of disease, and have already been accorded distinctive clinical names; others quite as distinct are not as yet so well known, and of others we may say that they are to be distinguished from other inflammations of the same structures not so much by their features as by their cause. Of all we may assert that they are infrequent; some, if we confine ourselves to well-marked types, are distinctly rare. We have divided these different affections into two groups: (1) those who go with acquired, humoral, or renal gout, and (2) those which depend upon the inheritance of structures damaged, or at any rate specialised, by gout in predecessors. It is needless to repeat that in almost all cases of acquired gout there is inheritance also, and that in many in which the disease is chiefly caused by inheritance, some modification and increase may have been derived from personal habits. Still, the difference between the two classes of affections is very marked. In the one attacks of a transitory nature are the rule, and these attacks are often acute and attended by much pain. In the second group, although a tendency to temporary recovery and recurrence is often observed, yet there is a great proneness to chronicity and persistence. The invasion is often insidious, but the disease is usually in the end destructive. In the former group we have placed hot eye, scleritis, recurrent iritis, and retinitis hæmorrhagica. All these are diseases of adult life. In the second group we have insidious disorganising iritis, relapsing cyclitis, certain forms of soft cataract, and perhaps some of primary optic neuritis. Not only are there clearly marked clinical differences between the two

classes of affections, but the difference in treatment is equally marked. In the first, the well-known measures against gout must be taken, a restricted regimen, alkalies, colchicum, aconite, and liberal counter-irritation. In the second we must use tonics, and although counter-irritants are here also often valuable, we cannot trust to any measure as really curative short of complete change of climate.

ON THE PROOFS OF GOUT.

It may be, perhaps, convenient to say a few words as to the kind of evidence which justifies a diagnosis of gout as the cause of any particular disease of the eye. In the case of humoral or acquired gout there ought to be the history of one or more definite attacks of joint inflammation usually of an acute character, and attended by redness and œdema, and followed by peeling; usually the great toe will have been the joint affected. Such patients will often state that they are very susceptible to the influence of beer and wine, and that malt liquor and some wines almost always cause indigestion, and make the urine muddy. These dietetic disturbances, to which as a test of gout attention was, I think, first claimed by Sir James Paget, are very important and valuable. If tophi are present in the ears or elsewhere they are of course conclusive. In a few cases we are justified in assuming the existence of humoral gout, although no paroxysm has ever occurred. If the dyspepsia be there, if the joints ache and prick after beer or wine, and if there be gout in relatives, we may confidently believe that it is present, although not yet declared. As regards the inherited form, we may take it as highly probable whenever parents or grandparents, or any one of them are known to have suffered definitely. If even uncles, aunts, brothers, or sisters, or cousins have suffered from true gout in early life, the belief that a family taint exists becomes very probable. The evidence must always be carefully sifted.

It will not do to take the statement of the patient without first carefully informing him as to the scope of the inquiry. Patients will often confess to gout who do not know what the word means, and a far more numerous class will hastily deny its history, although the facts, when correctly obtained, may be most conclusive. If, however, proper care be taken, and the patient, after being instructed, be allowed time for consideration—above all, if the inquiry be repeated after an interval, or if several relatives be interrogated, then I believe that in most cases truthful data will be obtainable.

It may be inquired as to the value of certain affections which may be considered to belong both to rheumatism and gout, as symptoms of the latter. Permit me very briefly to repeat my creed. I believe that the subjects of gonorrhœal rheumatism are in a very large majority of instances the inheritors of a gouty constitution, and that all the conditions usually classed as rheumatic gout are really, in most instances, dependent in a large degree upon like inheritance. Thus, if a patient has had sciatica or lumbago, if he shows *nodi digitorum* (osseous, not tophi), if he has suffered from chronic rheumatism affecting the smaller joints, I should think it fair to allow considerable weight to these facts as pointing to a taint of gout. Even the strictly rheumatic disorders, acute rheumatism itself, if it happens to the relatives of those who have had gout, lends support to the theory of family tendency to gout. It is an observation as old as the days of Heberden, and confirmed I am sure by daily experience, that the children of the gouty are more liable than others to attacks of rheumatic fever. In this we see another proof of the inheritance of structural proclivities, rather, I think, than of blood disorder or tendency to it.

CONCLUSION.

And now, gentlemen, as my last word, I do not know whether I have to defend myself in the eyes of any of

you from the charge of "seeing gout in everything." I am well aware that this diagnosis is a very easy one, and seductively ready at hand for the idle prescriber. I submit, however, that it has not been exactly in that temper that I have brought before you the statements which I have made this evening. My desire has been to state the issues explicitly, and to keep close to facts. Where statistics were admissible and obtainable I have had recourse to them. I may assert that I have said nothing but what has been based not only upon clinical observation, but upon clinical note taking, and the subsequent collation of cases. Nor, I contend, have I made any very sweeping statements. So far from my having exceeded the truth, my conviction is that when our clinical pathology shall be more advanced, and diseases more minutely classified, we shall in all probability recognise as gouty yet other maladies, and perhaps not a few beyond what I have claimed. It is a subject upon which scepticism is as irrational as credulity. That the gouty constitution exists, and is very common in our English population, that it is potent in the production of disease, and that it is remarkably hereditary, are facts which no one will doubt. In relation to the multiform diseases of the eye it must have a domain, and that an important one. To discover some of the extensions and limits of that domain has been the object of my best endeavours to-night.

ON A CASE
OF VERY LARGE
LYMPHO-SARCOMATOUS TUMOUR OF
THE TONGUE.

BY
JONATHAN HUTCHINSON, F.R.S.,
EMERITUS PROFESSOR OF SURGERY IN THE LONDON HOSPITAL COLLEGE.

Read June 9th. 1885.

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THE case which forms the basis of my present communication was under my care about four years ago. The entire tongue of a young man, only twenty-two, was then removed on account of a tumour which had been slowly growing for more than half his life, and which had at length attained such a size that it impeded swallowing and even threatened suffocation. I have purposely allowed a considerable period to elapse before submitting the case to the consideration of the Society, because I wished to have the evidence afforded by time as to the true character of the growth. Two years had passed by, and as the patient remained quite well, I had begun to hope that we might assume it as probable that the tumour was of an innocent character. I am sorry to say, however, that after this long interval these hopes have been disappointed.

The whole mass weighed after removal seven ounces. It was by far the largest tumour of the tongue that I have ever seen removed, and I believe the largest on record.

The subject of the case was a medical student, aged, at the time of the operation, twenty-two years. His father was a medical man, and the conditions had, consequently, been carefully watched from the first. No growth had been observed, and no morbid condition of the organ noticed, during the first few years of life, and his father having had occasion to make applications to the tonsils in childhood, felt sure that he should have seen it, had anything been present.

The first symptom that was observed was at about the age of ten, and consisted of a swelling in the left side of the organ, with a rough papillary growth on its posterior surface. Without causing any inconvenience except from its size, the tumour continued to increase, and when, in the latter part of 1880, I first saw the patient, the organ was fixed in the mouth by its mere bulk, and speech was difficult. At this time the tumour consisted of a large rounded mass, deeply embedded in the tongue, and wedging itself against the sides of the lower jaw. The tip of the tongue was free, and could be moved on the surface of the tumour, but with this exception the whole of the organ was involved. The tumour could easily be felt externally as a hard mass bulging downwards behind the chin. The mucous membrane of the tongue over the tumour was quite healthy, with the exception of the posterior two thirds on the left side, which presented a coarse papillary growth, not in the least painful, nor ulcerated, but which was continuous with the substance of the tumour beneath. The surface at this part was nodular, like the outside of a mulberry. Had it not been for this growth, which implicated the overlying parts, so moveable was the mucous membrane and the superficial layer of muscular structure on the tumour, that one might have been tempted to hope that it was encapsuled, and might possibly be shelled out.

I had had the advantage of the opinion of Sir James Paget, to whose kindness indeed I had been indebted for having the patient placed under my charge, and I subsequently obtained that of Mr. Savory and several other professional friends.

It was felt by all that the time must come when it would be absolutely necessary to operate, and, encouraged by the long duration of the case, we were all hopeful that the disease was not in any sense malignant. As to its precise character, however, no one ventured a confident opinion. The operation was not performed until nearly a year after I had first seen the patient, and in the interval the growth had increased considerably, and the inconvenience had finally become unbearable.

The operation was done on November 20th, 1881, and I had in it the able assistance of Mr. Waren Tay and Mr. R. W. Parker.

The patient having been put under ether a preliminary tracheotomy was performed. It would have been better to have done this without the anæsthetic, for as soon as insensibility commenced the mass fell back upon the glottis and the patient was in the utmost danger from asphyxia. It was impracticable to draw it forward sufficiently to effect relief, and, after a hurried completion of the tracheotomy, we were obliged to do artificial respiration for some time to restore animation. The patient having well rallied the pharynx was next plugged with sponge, and an exploratory incision was made into the substance of the tumour in order to ascertain whether or not it was encapsuled. Finding that it was not so, that it was very firmly fixed on all sides, and that it was quite impossible to get the finger either beneath or beyond it from the mouth, I at once made an incision through the lip and chin and cut the symphysis of the jaw. Having freed the muscular attachments and widely separated the two halves of the jaw, access was gained to the base of the organ. Having well isolated the mass, partly by scissors and partly by tearing, the wire *écraseur* was

applied over its base just in front of the epiglottis, and the removal completed. There was no trouble from hæmorrhage.

No difficulty occurred in the after treatment. The jaw united well and a rapid recovery followed.

The drawings (Plate V) give a good idea of the appearance of the growth, both on its surface and in section. It will be seen that the mammillated patch occupies an area larger than a crown-piece, and that its growths are at least a quarter of an inch in height. The tumour itself, as seen in section, was almost globular, and measured two and a half inches across. It completely replaced the substance of the tongue, with the exception of a portion about three fourths of an inch in length at its tip. The posterior two thirds of the growth showed a greyish fibrous structure, which was divided into loculi by white bands. The anterior part was red, not so hard, and much more vascular; but obscurely divided in like manner into lobes. On its surface the growth was everywhere bounded by a tolerably well-marked layer of fibrous tissue. On its surface in many parts, especially in front, were seen what looked like the cut ends of small muscular bundles. There were no cysts, and nothing that was conclusively erectile. The muscular bundles of the front part were inseparably attached to the growth.

One half of the tumour was sent to the College of Surgeons, where it was examined by Mr. Eve, the pathological curator, who also obtained the assistance of Dr. Klein. The other half was examined independently by my eldest son, who had the valuable assistance of Mr. Rickman Godlee in inspecting the microscopic sections. These observers all agreed in reporting that the growth was of the connective-tissue type, and probably a round-celled or lympho-sarcoma. Here and there in its substance muscular fibres, atrophied by pressure, could be demonstrated. A tendency to the development of well-formed fibrous tissue was a strongly characterised feature. The enclosed loculi were of very various sizes and contained cells, which had rela-

tively large nuclei and an ill-defined cell substance. Between the cells a fine reticular tissue was seen in parts.

The much enlarged papillæ were infiltrated with cells, not apparently differing from those in the alveoli beneath.

Between the papillæ in some of the fissures the deep epithelial cells had much enlarged and undergone granular degeneration. Everywhere there was a sharp line of separation between epithelium and corium. (Pl. VII.)

It will be well before attempting to discuss the clinical relationships of this case to give its termination, and also to cite any evidence which can be obtained from the records of other cases more or less resembling it.

After his recovery my patient, who was a gentleman of great pluck, at once returned to his medical studies at Belfast. From time to time I heard of his being quite well, and two years after the operation I heard that he was intending to present himself for examination at our London College, and that I should then have an opportunity of seeing him and hearing how well he could make himself understood. About six months later, however, I had the disappointment of being informed of his death. A growth had very rapidly sprouted up in the floor of his mouth and had in a short time brought about the fatal event, partly by pressure and partly by hæmorrhage. I did not hear of the recurrence until after his death, and there was no post-mortem. I believe that there was no evidence of gland disease.

I am not able to produce either from the records of other surgeons or from my own experience, any case which is an exact parallel to the one which has been described. The following cases, however, in a fragmentary way, some from one aspect, and some from another, seem to throw light upon its probable nature. We may note that we have to deal with a tumour which invaded the substance of the organ in which it was placed; which

began in very early life ; which was attended by coarse mammillation of the surface ; which continued to grow painlessly and very slowly through a long series of years ; which never produced gland disease ; which recurred locally after removal, and which under the microscope presented the characters of an infiltrating lympho-sarcoma, with dilatation of blood-vessels.

The first case to which I shall refer is that of a child, aged three, in whom a mammillated growth on the posterior part of the tongue had been present from birth. It had caused no inconvenience but was slowly increasing. I excised it by means of the cautery, and the child, I believe, remains well. It did not go deeply into the substance of the tongue.¹ The method of removal destroyed the specimen for microscopic purposes.

Specimen 1067A in the Museum of the College of Surgeons is the tongue of an infant in whom similar conditions, but on a much larger scale, were present. It was shown by Dr. Hickman, in 1869, at the Pathological Society, and is described in vol. xx of the 'Transactions.' The infant was born with a mammillated tumour on the left side of the dorsum of its tongue as big as a plum stone. It was sessile and extended from the line of the circumvallate papillæ back to the epiglottis. Owing to its peculiar position, rather than its size, it caused death by suffocation within sixteen hours of birth. A committee appointed by the Society reported that it consisted of hypertrophied gland follicles and ducts with an intervening matrix of nucleated connective tissue. Its blood-vessels were large and numerous, but there was no definite erectile tissue.

Mr. Liston has recorded a case in which he operated on a lad, aged nineteen, for a tumour of the tongue which had been present from birth. In some respects this case resembles the ordinary macroglossia of young children, but in others it differs from it, and is suggestive rather of

¹ I possess a coloured sketch illustrating this case. It was produced at the Society's meeting.

a nævoid mole. The tongue filled the mouth and projected over the lips. It was elastic and compressible and its surface was crossed by large venous trunks. The papillæ over it were much enlarged, and granular points were numerous. There was an ulcerated fissure near its centre which frequently bled. Mr. Liston tied the lingual arteries and then ligatured the whole tongue. Unfortunately the patient died of pyæmia.

My next caso (Plate VI) is clearly very like Mr. Liston's excepting as to size. Its subject was, when I first saw him in 1872, a lad of twelve. Ho is now a porter at Woking Junction aged twenty-four, and in good health. The state of his tongue has not materially changed. Tho portrait shows considerable hypertrophic onlargement of the organ chiefly of its left half, and tho surface is covered with mammillated and granular points, just such as Mr. Liston described, and such as were prosent in the caso which is the chief subject of this paper. Some of them were vascular, like parts of a venous nævus. The condition had been noticed soon after birth and it increased somewhat in growth. He was sent to the London Hospital from Farnham when he was twelve, because it had increased somewhat, and with a view to operation. I kept him under observation a few weeks and finding that the condition caused the boy no inconvenience whatever I deferred any interference. My decision has been justified by the fact that he has got no worse. This case seems to differ from that which my paper chiefly concerns in that there is no tendency to continued growth in the substance of the organ. It is very possible, however, that this tendency may yet declare itself.¹

The next case was a child under the care of Mr. Waren Tay in the London Hospital. The tumour was congenital and deeply placed in the substance of the left half of the

¹ The man who is the subject of this case attended the meeting for inspection. Further examination made it certain that although, as may be seen in Plate VI, the growth appears to involve both halves, it is really limited to the left, and only encroaches on the right by bulging over.

tongue, bulging both below and on the dorsum. The tip of the tongue was free, there was no papillary hypertrophy, and the tumour, although partly solid, consisted chiefly of cysts. I believe the child died after an operation for its removal.

Closely parallel to this, and presenting also features of similarity to my own, is a case which I find narrated by my friend Dr. George Brady, of Sunderland, in the 'Medical Times and Gazette' for 1867. An infant had, at birth, a bluish looking tumour under and in its tongue, which was taken for a cyst. It was punctured freely, and almost fatal bleeding followed. At the age of fourteen Dr. Brady attempted the removal of the tumour, which was then a softish solid. It was sought to excise the growth from under the tongue without removal of the latter, but the tumour was so ill-defined and the bleeding was such that the operation was not perfectly completed. The boy, however, recovered and at the end of several years was still well and without any new growth.

A case which has been recorded, in excellent detail, by Mr. Henry Arnott, in the 'Pathological Transactions' (vol. xxiii), well illustrates the combination of congenital hypertrophy of various different structures in the same organ. The infant was fourteen months old when he was operated on by Mr. Simon in St. Thomas's Hospital. The tongue was of very great size and lolled out between the stretched lips. It had been large from birth and had hindered sucking. The child died of pneumonia three weeks after removal, by the *écraseur*, of the greater part of the tongue. Mr. Arnott records that "there were present, first, a *nævroid* affection of the blood-vessels; secondly, a thickening and induration caused by a long-continued sub-inflammatory state; and thirdly, a general enlargement of the lymphatics. Rather large irregular spaces, with very thin walls, and mostly with no visible contents, were met with in every section. Some of these spaces were filled with blood, but for the most part they contained only clear fluid with a few vesicular bodies—swollen

epithelial cells—in contact with their walls, possibly dilated lymphatics.”

In none of the cases which I have quoted, excepting my own, was there any proof of the presence of any solid growth which might deserve the name of lympho-sarcoma. In Mr. Brady's case the growth was solid and very possibly of this nature; but it was not so diagnosed, and in Mr. Liston's we have no microscopic examination at all. In several of the others we must remember that the child died after operation in early life and thus no opportunity was afforded for the further developments which took place in mine. The only recorded example of sarcoma, so diagnosed, which I have been able to find, is one published by Professor Jacobi, of New York, and quoted, with valuable criticism and additional facts, by Mr. Butlin in his 'College Lectures.' In this instance the tumour was the size of a hazel-nut at birth, and as big as a walnut when, at three months old, it was removed. It was a firm, rounded, elastic mass, deeply furrowed on its surface. Its section was uniform, excepting that in its centre was a small cyst. Its external portions contained muscular tissue, but its chief structure was that of a spindle-celled sarcoma. There were some round-cells, but not much intercellular substance. The child in this instance was well five months after the operation but nothing more is known of it.

The last case which I shall quote is one published by Mr. Folker, of Hanley. In it a large solid tumour which had been growing for twenty years was successfully removed from the tongue of an adult man. It was diagnosed as “fibrous.”

The cases which I have adduced justify, I think, the conclusion that my case ought to rank as one of a group in which hypertrophic structures, present in the tongue at the time of birth, subsequently take on a more or less erratic development. These congenital excesses are probably analogous to moles and nævi of the skin; and in the tongue, as proved in several cases, dilatation of blood-

vessels and of lymphatics is a conspicuous element in the growth. We know that moles of the skin often show vascular as well as papillary and fibrous hypertrophy. All the structures, in irregular proportions in different cases, are involved much as we have noted in the series which I have this evening produced. Moles and nævoid moles of the skin sometimes take on growth tendency, and this may happen at any period of life, and sometimes, as is well known, melanotic sarcoma may occur in them. In some rare instances fibroid hypertrophy in moles shows a definite tendency to recur. I once removed for a young lady a huge pigmented and papillary mole which grew on the pubes and involved the labium. It was impossible to remove the whole. A tendency to subsequent growth was most definite, and second and third operations were required. The subcutaneous fibroid hypertrophy produced a dense hard mass an inch and a half in thickness. The pathological processes displayed in this case were probably very similar to those which occurred in the tongue in the case which I have had the honour to bring before the Society.

In conclusion a few words may be said as to the surgical aspects of the case. It might, perhaps, have been better, as regards the prospects of immunity, if I had operated when the patient first came under my notice. My reasons for delay were, that the growth had already attained such a size that the operation must of necessity be a very formidable one, and that as it seemed highly probable that the tumour was innocent so it was wise to put off the danger as long as possible. Guided by our experience in this instance, I should certainly in any similar case—in which a tumour in the tongue, however apparently innocent, was steadily growing, however slowly—be inclined to advise an early removal. It may be doubted, however, whether a patient in whom such a growth caused neither pain nor inconvenience and had been present for years, would be inclined to submit early to an operation involving the loss of the entire tongue. Such

would have been the conditions under which any surgeon who saw this case before I did would have had to advise.

(For report of the discussion on this paper, see 'Proceedings of the Royal Medical and Chirurgical Society,' New Series, vol. i, p. 458.

DESCRIPTION OF PLATES V, VI, AND VII.

(On a Case of very large Lympho-sarcomatous Tumour of the Tongue, by JONATHAN HUTCHINSON, F.R.S.)

PLATE V.

Case of Lympho-sarcomatous Tumour (see p. 311).

FIG. 1.—The tongue, after removal, seen from before.

FIG. 2.—The same seen from the side.

FIG. 3.—A longitudinal section of the tongue from tip to base.

PLATE VI.

Case of Congenital Unilateral Hypertrophy of Tongue (see p. 317).
(A papillary, lymphatic, and nævoid mole.)

PLATE VII.

Microscopical Sections from Tongue, figured in Plate V.
(Drawn by Mr. J. Hutchinson, jun.)

FIG. 1.—Section of the surface of the tongue made through part of two of the large “papillæ” which covered it. The epithelial layer was everywhere well defined, and here presents nothing abnormal; the subjacent layer is very vascular and somewhat infiltrated with lymphoid cells, whilst towards the lower part these show a tendency to be arranged in irregular alveoli.

a. Horny layer.

b. Rete mucosum.

c. Vessels.

d. Deep part beginning to show an alveolar arrangement.

FIG. 2.—A section characteristic of the great mass of the tumour, showing large rounded alveoli filled with lymphoid cells of uniform size, enclosed by bands of fibrous tissue and intersected by finer strands of the latter.

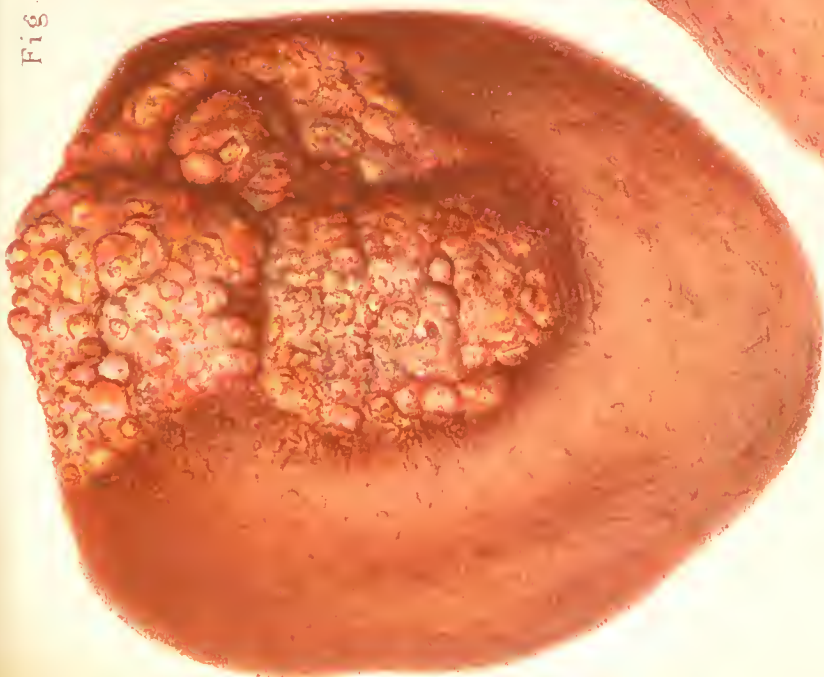


Fig. 1.



Fig. 3.



Fig. 2.





Congenital unilateral Hypertrophy of tongue.

Fig. 1.

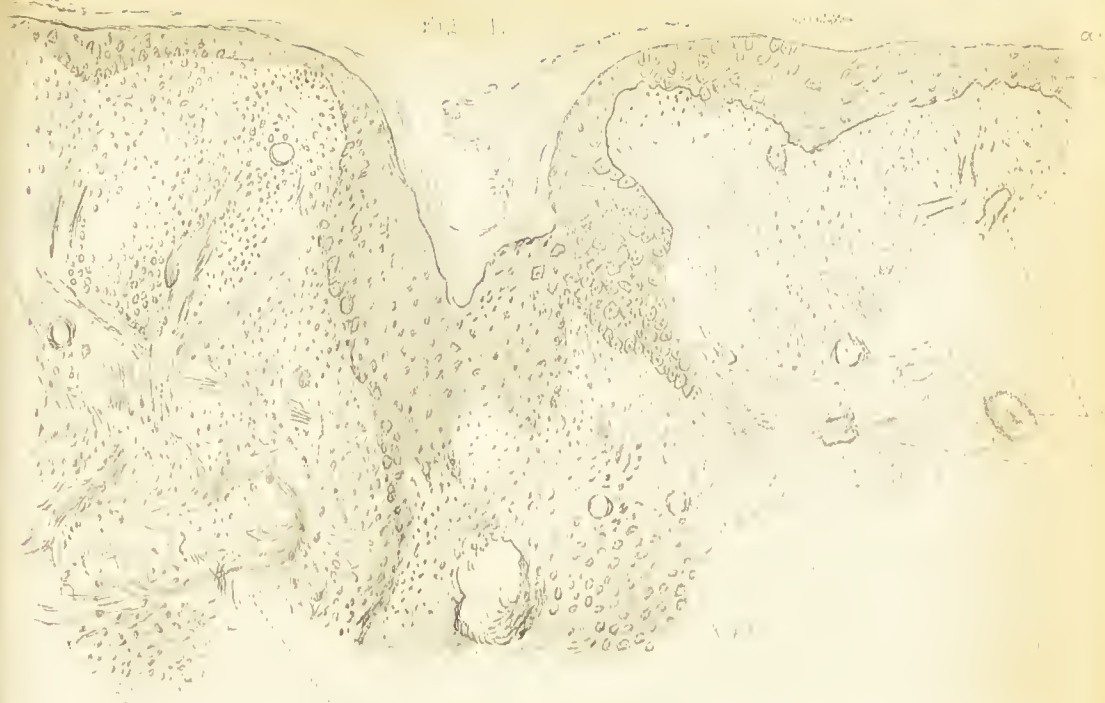


Fig. 2.







